

ABSTRACT

Title of Dissertation: **ESSAYS IN GENDER AND DEVELOPMENT**

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This dissertation consists of three essays at the intersection of gender and economics in developing countries. In chapter [1](#), I study the economic implications of a particular cultural practice: cousin, or consanguineous, marriage. One sixth of all marriages in Egypt are between first cousins, but there are important differences in the characteristics of individuals who select into such relationships relative to those who marry non-relatives. To measure the causal impact of the practice on socioeconomic outcomes abstracting from selection, I instrument for the probability of marrying a cousin using exogenous variation in family structure, and use weak instrument robust methods to estimate parameters and evaluate statistical significance. I find that individuals who marry a cousin because of exogenous attributes of their natal family structure are further in age from their spouse, predominantly driven by older men marrying cousins. I also find that women married to cousins receive higher levels of marital transfers that give them bargaining

power within their marriages, likely as compensation for their spouse's attributes. This contrasts to patterns for those who select into cousin marriage; those individuals are younger at the time of marriage, match with partners closer to their own ages, and have no differences in the level of marital transfers exchanged. The contrast between OLS and IV results suggests that selection into cousin marriage may be motivated by anticipation of not matching on the wider marriage market, credit constraints, or the desire to consolidate property within the extended family.

In chapter 2, I present baseline statistics from an experiment which examines the impact of random job offers on women's experiences of intimate partner violence in Bangladesh. This paper build on a larger study which aims to increase women's labor force participation and use of mobile money services. I collect supplementary data on women's experiences of intimate partner violence, men and women's agreement with conservative social norms, and second order beliefs regarding their community's sanction of intimate partner violence. I validate survey measures of intimate partner violence with a list randomization elicitation. I also present results from two incentivized decisionmaking activities conducted at baseline. I specify the outcomes I plan to test once endline data is available, as well as the econometric specifications I will use. Finally, I present power calculations using baseline data to determine the smallest effect sizes I can detect.

Finally, in chapter 3, I study the impact of an exogenous negative shock to labor demand for female migrants within Bangladesh. I use a difference in differences strategy and compare outcomes between districts that have a history of sending migrants with those that do not, before and after the shock. I find that migrants respond to the initial shock and return to their households rather than remain unemployed in Dhaka, and that at least some of these women marry. I see no decrease in the level of investment in children's human capital, which suggests households do not revise their perceptions regarding the returns to education, and have access to other tools to

smooth consumption. Finally, I see no changes in the daily agricultural wage rate for women in the years after the shock. I lack data on several important margins of adjustment which would allow us to discern the mechanisms behind the effects.

ESSAYS IN GENDER AND DEVELOPMENT

by

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Dedication

To Amma, Appa, and Anuradha

Acknowledgments

I owe everything to my family. My parents have sacrificed so much to give me incredible opportunities to further my education. Their unconditional love and support mean the world to me. My sister Anuradha has been my biggest cheerleader from the start. I'm so grateful to have had her friendship, guidance, and support. To my grandmothers, Lalitha and Parvathi—their unwavering belief in the potential of their grandchildren has motivated all of us throughout our studies. I love you all so very much.

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Chapter 1: Consanguinity and Marital Transfers in Egypt

1.1 Introduction

The choice of a spouse affects the well being of both parties in a marriage. In many developing countries, where the exchange of monetary transfers at marriage is prevalent, who one marries further affects well being through the amount of marital transfers given to the couple. How these transfers are allocated between the bride and the groom has additional implications for women's welfare. In many low income settings, marital transfers can form the primary source of wealth for women, given their low levels of labor force participation and smaller inheritance shares. An increase in the amount of money a woman is able to control can allow her to make more decisions independently of her husband, and can improve her options outside of her marriage.

An additional way that marital transfers affect women's well-being is unique to the Egyptian context I study. Nearly all marital transfers given to the couple at the time of marriage are considered the bride's property in the event the marriage dissolves. If a woman believes her husband is not upholding his end of the marital contract, she may threaten to demand the value of all transfers she is due. Her husband has the option to incur a large financial penalty, or attempt to make amends. Hence, in Egypt, marital transfers explicitly improve a woman's bargaining power within her marriage.

In this paper, I consider how marital transfers and women's welfare are affected by one dimension of the complex choice of who to marry: the decision of whether or not to marry a first cousin. Consanguineous, or cousin, marriages are prevalent in countries with high Muslim populations. Pakistan has the highest rate, with over 60% of individuals marrying first cousins. In the Middle East and North Africa, the practice is fairly commonplace, ranging from 15-40% [21]. In my sample in Egypt, 16.7% of marriages are between first cousins.

I focus on the socioeconomic implications of first-cousin marriage: its impact on the timing of marriage, the size and division of marital transfers, and the associated levels of women's empowerment. Quantifying the impact of cousin marriage is difficult given the substantial degree of selection into the practice. Characteristics which are associated with a higher probability of marrying a cousin may also directly affect the timing of marriage and amount of marital transfers exchanged. For example, individuals who face high search costs in the marriage market may choose to marry a cousin due to the relative ease of searching among a smaller pool of known cousins. However, the same factors that make searching for a spouse costly may make searching for a job costly, and affect their ability to finance marital transfers. These characteristics are unobserved by the econometrician, and an OLS regression would not be able to separate the effect of cousin marriage from the impact of factors that influence selection.

In order to identify the impact of cousin marriage abstracting from selection, I leverage exogenous variation in family structure as an instrument for cousin marriage. In the presence of strong social norms against men marrying older women, the set of an individual's parents' siblings whose children are more likely to be an age and gender appropriate match is different for men and women. Specifically, a parents' older siblings are more likely to have a son who is older than a woman, while the set of parents' younger siblings is most likely to have a daughter

who is younger than a particular man. Given that historically the practice was more common along the male line of the family, I use variation in father's birth order to create the instrument. The interaction between an individual's gender and their father's birth order identifies the set of individuals who marry a cousin because more of their father's siblings were likely to have age-appropriate matches given that individual's gender. I present IV point estimates from several estimators: Andrews and Armstrong [11]'s unbiased estimator and the Fuller estimator, which are more robust to weak instruments, as well as 2SLS. To assess the statistical significance of my estimates, I present p-values from the Anderson-Rubin test, which is correctly sized regardless of instrument strength, unlike t-tests.

IV estimates of the impact of cousin marriage show that men who marry a cousin because of their family structure delay marriage, and face a larger age gap with their spouses than if they had not married a cousin. I also find that cousin marriages give women larger marital transfers which increase their bargaining power within their marriages, compared to unions between unrelated individuals. While I find no statistically significant differences in indices that measure women's decisionmaking ability and physical mobility after marriage, I do see differences in several components of these indices. Women are more likely to have decisionmaking abilities about household food and their own medical care, but are less likely to make decisions about visiting family and friends, or travel alone to the market.

The causal effect of cousin marriage I identify through my IV strategy is quite different than the OLS estimate, which includes differences in outcomes due to selection into cousin marriage. If I had estimated results using OLS, I would have found that consanguinity hastens the timing of engagement by a year for both men and women who married cousins compared to if they had married a non-relative. OLS results also show that engagements last 30% longer in cousin

marriages, but I do not find precisely estimated differences in the level or allocation of marital transfers. Finally, I find that OLS predicts consanguineous couples are more likely to live in a joint household, but does not predict any differences in women's decision-making abilities or physical mobility.

While the IV effect sizes I estimate are large, likely a consequence of instrument weakness, the results from the Anderson-Rubin test allow us to correctly reject that the true effect of cousin marriage on the age gap between spouses, and transfers related to women's bargaining power is zero. Furthermore, comparing the IV and OLS results suggests that the unobserved characteristics which motivate selection into cousin marriage are negatively correlated with the timing of marriage: individuals who opt into cousin marriage do so at an earlier age than they would have if they married a non-relative, and do so at an earlier age than individuals who are motivated by the instrument. These characteristics are also negatively correlated with marital transfers that increase a woman's bargaining power within her marriage; while OLS estimates suggest there are no differences in these marital transfers, IV results suggest that women receive higher valued jewelry and deferred dowry when they marry a cousin.

These patterns of selection could indicate that cousin marriage is a response to credit constraints, or anticipation of not making a match on the wider marriage market. Among the population of individuals induced to marry a cousin by their family structure, cousin marriage appears to be an option exercised by men who either do not make a match on the wider marriage market after engaging in a lengthy search, or who otherwise delay marriage. The female cousins they marry do appear to be compensated for the less desirable characteristics of their spouses. These marriages may also have been easier to contract due to lower search costs or better information due to existing family networks.

Many existing applied papers studying consanguinity examine the factors which affect its prevalence. I go further and study the effect of cousin marriage on socioeconomic outcomes. I provide both descriptive evidence of the types of selection that motivate cousin marriage, as well as causal evidence of the effects of consanguineous marriage on the timing of marriage, marital transfers, and women's empowerment. I also provide empirical evidence on the impacts of cousin marriage from a geographic region where the practice is prevalent, but which operates under different customary marital transfers. Much of the existing literature on consanguinity studies the practice in South Asia, where dowry (a transfer from the bride's family to the groom) is commonly exchanged. These papers find that consanguinity is associated with credit constraints, and that dowries are lower among cousin matches. In Egypt, where most marital transfers are ultimately property of the bride, I find that cousin marriage increases the amount of marital transfers which are held by the bride, though the total amount of transfers exchanged remains unchanged.

The rest of the paper continues as follows: in section 1.2, I describe marriage in the Egyptian context, including a detailed description of marital transfers. In section 1.3, I review the literature on the economics of consanguineous marriage, and the economics of marital transfers. Next, I describe the datasets I use in section 1.4, and in section 1.5, I describe my identification strategy. Section 1.6 presents OLS and IV results, as well as a discussion of the types of selection that may be driving OLS results. Finally, in section 1.7, I conclude.

1.2 Marriage & Marital Transfers in Egypt

Social norms in Egypt dictate that marriages be accompanied by several monetary and in-kind transfers from the bride and groom's families, some of which are nominally given jointly to the couple, and others which are always the sole property of the bride. These large inter-generational transfers of wealth from parents to their children are worth approximately four years of earnings. There are six main types of marital transfers: shabka, dower, afsh, gihaz, mo'akhar, and the value of housing given to the couple. The value of these transfers are negotiated before marriage—usually after both parties declare their intention to marry during the informal engagement (*qirayet fatiha*), but before the formal engagement (*shabka*).

The first transfer exchanged is the shabka, a gift of gold jewelry that is presented to the bride by the groom and his family at the couple's formal engagement. A nonzero value of shabka is exchanged by 95% of couples in my sample, and the mean value is E£8,621.78. The next set of gifts are exchanged after the marriage contract (*katb kitab*) is signed: the dower, afsh, and gihaz. The dower is a lump sum transfer given from the groom's side directly to the bride. The expected value of a dower in my sample is E£2,728.57, however, only 31% of households report having exchanged a nonzero amount of dower. Conditional on exchanging a positive amount of dower, the average value is E£8,776.86. The gihaz, or the bridal trousseau and household goods, and the afsh, or household appliances, total to E£19,048.52 and E£27,480.85 respectively. 96% of respondents in the sample report exchanging a nonzero amount of these transfers. The couple is traditionally provided accommodation by their parents, either in the form of a purchased house or apartment, or advance money given to secure a rental. Housing expenditures on the couple are valued at E£28,088.36 on average. The last transfer, the mo'akhar, is known as a *deferred* dower,

and is only given to the bride upon divorce, or the death of her husband. 81% of households in my sample report recording mo'akhar, valued at E£7,005.21.

Both the bride and groom's families make significant outlays to finance marital transfers. The bride's family spends on average E£24,976.56 on average, paying for 65% of the gihaz, 40% of the afsh, 7.5% of the housing expenditures, and 18% of the celebrations. The groom's family is also responsible for the entirety of the dower, shabka, and mo'akhar, and spends E£65,846.19 on average. In general, the size of marital transfers depends on the characteristics of the couple and their families, including whether or not the marriage is consanguineous. These transfers are economically significant; their total value is equivalent to approximately 4 years' worth of earnings for the average man in Egypt [93].

Some marital transfers are nominally given to the bride, and some are given for the couple to use jointly. However, with the exception of housing, the ownership of all marital transfers reverts to the bride upon dissolution of the marriage [34, 88]. Women in Egypt do not have the same ability to dissolve a marriage as men do. Yet, women's ownership over marital transfers can allow them to exercise bargaining power without needing to formally divorce their husbands. El-Kholy [34] shows how many marriages include an additional document, the ayma, which is an inventory of the dower, shabka, mo'akhar, and all goods given in the gihaz and afsh, signed by the groom in the presence of two witnesses who are also guarantors. If the husband fails to fulfill his end of the marital contract in any way, the bride can demand all the goods listed in the ayma to either force her husband to fulfill his obligations, or dissolve the marriage and compensate her. The ayma is enforced by both the community and courts of law. Often, at the behest of the bride's family, the ayma is not an accurate record of the gihaz and afsh, and either inflates the value of the items exchanged, or includes items not yet purchased. This provides the bride additional

leverage in the event of marital discord: if a man cannot produce each item listed on the *ayma* in court, he may be forced to give his wife the monetary value of the missing items, or be subject to incarceration if he is unable to do so [76]. One exception to this practice comes after the *khull* reforms in 2000: women may choose to forgo any claims on marital transfers in exchange for an expedited legal divorce.

1.3 Literature

1.3.1 Consanguineous Marriage

Papers on consanguineous marriage broadly fall into two categories: those which study the conditions from which the practice arises, and those which attempt to estimate the causal impact of consanguineous marriage. Two papers fall in to the former category: Do et al. [31] and Jacoby and Mansuri [57]. Do et al. [31] conceptualize a model in which consanguineous unions are a solution to a particular moral hazard problem in the marriage market: that parents cannot credibly commit to investing in their children's marriage consistently after the wedding. Households have two remedies: contribute high initial transfers prior to marriage in lieu of a stream of smaller transfers ex post, or, attempt to enforce commitments continuously after marriage. When in-laws are siblings, they are socially close enough such that the cost of monitoring and enforcement is low, making consanguineous unions a viable solution to this free-riding problem. Very poor or credit constrained households in particular find cousin marriage preferable to financing a large lump sum transfer ex ante. The authors present suggestive evidence from Bangladesh, showing that cousin marriages are more prevalent among lower wealth households, as proxied by landholdings. My results do not completely rule out credit constraints as a motivation for cousin

marriage in Egypt: I find that men who marry cousins do so at an older age. It may take longer for credit constrained individuals to finance marital transfers, leading to a delay in the age at marriage. However, IV results show that women in consanguineous marriages have higher levels of marital transfers that give them bargaining power in their marriages, suggesting the cousins they marry give higher transfers than the men they otherwise would have wed. OLS results also do not rule out that marital transfers in consanguineous marriages are the same as those exchanged in exsanguineous unions.

Jacoby and Mansuri [57] show how marriage between close relatives can solve another commitment problem in the marriage market: that grooms cannot ex ante commit to not physically harming their brides after marriage. In Pakistan, the solution adopted to safeguard daughters is the custom of *watta-satta*, or brother-sister exchange marriage, where one sibling pair is married to another. As over 60% of all marriages in Pakistan are between first cousins, *watta-satta* between cousins adds another level of monitoring to safeguard daughters' welfare. The authors theorize that the practice functions as a commitment device to reduce intimate partner violence; since each household has a daughter from the other, mistreatment of one implies retaliation on the other. To avoid selection into the practice of *watta-satta* that may be correlated with the experience of intimate partner violence after marriage, the authors instrument for *watta-satta* marriage using the individual's sibling composition, and find that conflict between spouses decreases in such marriages. While my instrument is similar, I use variation in parents' siblings (father's birth order) rather than an individual's own siblings. An individual's parent's siblings are less likely to be reflective of the individual's human capital, which would directly impact their marital outcomes. While I lack data on the incidence of intimate partner violence, I instead study related outcomes that occur at a higher frequency: women's physical mobility and decisionmaking

capabilities in their marriages.

Several empirical papers use exogenous shocks which change household endowments to illustrate the role credit constraints play in cousin marriage. Work by Mobarak et al. [74] presents reduced form evidence on how wealth shocks to households help determine their choice to contract children to cousins. The paper looks at the impact of a large embankment construction project—a wealth shock to households living on one side of the embankment, who were then able to cultivate crops in multiple seasons—on the prevalence of consanguineous marriage. The authors find a decrease in rates of consanguineous marriage in villages that were protected by the embankment, after its construction. This suggests that prior to the embankment’s construction, cousin marriage was a response to credit constraints, and was no longer optimal once the constraints were eased. Das and Dasgupta [28] find a symmetric response to an increase in credit constraints. In the aftermath of the Gujarat earthquake in 2001, rates of cousin marriage increased in districts more affected by the quake than those which faced less damage. Jarallah [58] studies the impact of armed conflict on the probability of choosing a spouse within or outside of one’s family. The evidence appears to be mixed: the authors find that in Lebanon, women of marriageable age during the conflict were more likely to choose spouses outside of their families, but in Palestine, women were more likely to marry cousins. If credit constraints also play a role in Egypt, I should expect to see marital transfers decrease in consanguineous marriages, and possibly delays in the timing of marriage.

Finally, there is a small empirical economic literature on the impact of consanguineous marriage on children’s outcomes. Work by Mete et al. [72] looks at the impact of cousin marriage on children’s nutritional status and cognitive ability. The authors instrument for marrying a cousin using grandparents’ landholdings, theorizing that households with larger landholdings would be

able to finance better, unrelated matches for their children. They find that children whose parents are cousins are indeed shorter for their age, scoring about a full standard deviation lower than children of unrelated parents. This large effect may be due to the fact that wealth itself has an impact on children's health outcomes, aside from its impact on cousin marriage.

1.3.2 Marital Transfers

Most cultures where consanguineous marriage is prevalent also exchange transfers at the time of marriage. As such, the literature on consanguineous marriage overlaps heavily with the literature on marital transfers. Economists conceptualize marriage transfers as a market clearing mechanism. [Anderson's 2007](#) seminal paper on the economics of dowry and brideprice provides a comprehensive description of these transfers throughout history. She posits that brideprice is prevalent in contexts where women participate intensively in agriculture, as well as those where polygyny is common. Dowry payments arise when grooms have better access to economic opportunities and brides compete for marriages to wealthier grooms [9]. I study a context where dower, a form of brideprice given directly to the bride, is exchanged. In this framework, grooms in Egypt are competing for brides. If an OLS regression shows that cousin marriage increases dower or other transfers that ultimately accrue to the bride, it may show that men who select into the institution are still competing for brides. If the opposite holds, it could suggest that more women are selecting into consanguineous marriage, and are competing for husbands, as opposed to the rest of the market.

Several papers study the relationship between brideprice payments and women's welfare. Lowes and Nunn [70] study the impact of brideprice on outcomes related to women's welfare

in a sample of 318 couples in the Democratic Republic of Congo. While not causal, their findings suggest that higher brideprice could be associated with better outcomes for women. Larger brideprices are paid to women with higher levels of human capital, and are associated with more progressive beliefs regarding intimate partner violence and higher self-reported happiness for women. Finally, work by Ashraf et al. [16] finds that school construction programs which increase individuals' educational attainment only impact girls' education if they are from communities that practice brideprice. The prevailing direction of marital transfers affects parents' incentives to invest in their sons and daughters, and is an important factor in considering the impact of different policies. Similarly, the direction of marital transfers will partially determine the effect of consanguineous marriage on couples' outcomes, and may explain why results in Egypt are markedly different from the impact of consanguinity on marital transfers in South Asia.

The incentive to receive a marital transfer—or delay giving one—may affect the timing of marriage, especially in the event of a negative income shocks. Corno et al. [27] model how a household's decision to marry a family member changes in the event of a negative income shock. The authors show that the sign of the effect depends on the prevailing direction of marriage payments. They first demonstrate that a negative income shock causes delays in women's marriage in societies that exchange dowry, and hastens it in those that exchange brideprice, as the marginal utility of consumption is higher during the shock. When the authors allow grooms' households to also be affected by the income shock, the effect on marriage timing depends on which party is more sensitive to the shock. Empirical analysis using rainfall shocks confirms the predictions of the model, and further suggests that early fertility is an additional consequence of negative income shocks in cultures that exchange brideprice. Consanguinity may be another response to negative shocks; a negative income shock in the year a groom plans to marry may motivate him

to marry a cousin instead of an unrelated woman, especially if a cousin would be more likely to accept a lower immediate transfer. Indeed, delays in the timing of may be the result of negative financial shocks, rather than persistent credit constraints. I control for correlated shocks by including fixed effects for an individual's year of marriage in all regression specifications, but I do not observe whether a household faced a particular shock that changed marriage plans.

1.4 Data

I use data from the Egyptian Labor Market Panel Survey (ELMPS), collected by Egypt's Central Agency for Public Mobilization and Statistics (CAPMAS) and the Economic Research Forum (ERF). The first round of ELMPS was conducted in 1998 with a representative sample of 4,816 households across the country. The three following rounds conducted in 2006, 2012, and 2018 survey the original households from 1998, any households that split from previously surveyed households, and a refresher sample of new households. Work by Assaad and Krafft [\[17\]](#) shows that attrition between rounds is uncorrelated with observable characteristics, and that the sample remains representative of the Egyptian population.

To construct the sample I use in my analysis, I begin with the universe of married individuals in the 2006, 2012, and 2018 rounds of the ELMPS. I restrict my sample to individuals whose parents were surveyed in either the 2012 or 2018 survey rounds. For individuals currently living with their parents, I collect information on parents' birth order from the same survey round. Else, I use the individual's panel identifier to link them to prior rounds, and identify the last round in which they lived with their parents. After identifying the individual's parents' unique ID, I link the parents with 2012 or 2018 round to collect information on their birth order and other demo-

graphic variables. Appendix tables [A1.1](#) and [A1.2](#) show from which rounds each individual and their parents' information are gathered.

Information about each marriage is recorded identically for both spouses in the 2012 and 2018 rounds of the survey. In the 2006 round, some married individuals are missing information which their spouse reports, in which case I assign this information to the spouse who is missing the data. I am able to link information on a parents' birth order with their children's marital outcomes due to the panel structure of the data: when children split from the original household after marriage, I observe their marital outcomes. However, these children's spouse's origin family was most likely not in the data. As such, I only observe information on parents' birth order for one individual in every married couple. In the 206 cases where both the husband and wife have information on parents' birth order, I ensure that the marriage is not double counted in the data by randomly dropping either the husband or wife.

My final sample has 6,217 individuals who are the children of 4,693 unique fathers. I cluster all analyses at the father level to take into account correlation in error terms between siblings. I present summary statistics in table [1.2](#), split by type of marriage. Overall, 16.7% of my sample is married to a first cousin. Half my sample is female, and 37.6% live in an urban area. Women are engaged at 20 years of age on average, while men are engaged at 25 years of age on average. Engagements last approximately a year before a marriage ceremony is conducted. This sample only includes engagements that ultimately resulted in marriage, and does not account for prior engagements which did not culminate in marriage. Parents come from large households; on average, an individual's father is the third of five siblings in their birth household.

The data contains detailed information on the size of each marital transfer, and the pro-

portion of the transfer contributed by the bride and groom's sides. Additionally, I have detailed household data, including demographic information and measures of wealth.

1.5 Identification

1.5.1 Ordinary Least Squares

I first estimate the relationship between cousin marriage and various socioeconomic outcomes using the following ordinary least squares specification:

$$Y_{if} = \alpha_1 + \alpha_2 \times \text{Married First Cousin}_{if} + \mathbb{X}\mathbb{A} + u_f$$

where Y_{if} represents the outcome for individual i from father f , and $\text{Married First Cousin}_{if}$ takes on the value 1 if the individual married a first cousin, and 0 if not. I include a vector of controls, $\mathbb{X}$, which contains the individual's gender, number of siblings, geography, year of birth, education, father's wealth, father's education, and father's siblings¹. Differences in the outcome Y_{if} both due to the causal effect of cousin marriage, as well as the effect of selection, are captured by α_2 . Comparing OLS results to IV results can help shed light on the factors that motivate selection into cousin marriage.

¹As a robustness check, I estimate OLS results using different combinations of controls. Results suggest that observable characteristics explain some of the differences in outcomes between consanguineous and exsanguineous couples, but that there are still likely unobservable factors at play. Tables available on request

1.5.1.1 Hypothesized OLS Results

The bias in OLS depends on the correlation between unobserved variables positively correlated with consanguinity and the outcome of interest. The sign of the OLS coefficient depends on the sign of the true OLS effect and the sign of the bias. In table 1.1, I present the hypothesized OLS coefficient for the effect of cousin marriage on various socioeconomic outcomes under different unobserved motivations for cousin marriage.

I consider four main motivations for selection into cousin marriage motivated by the literature, and discuss the expected signs of the OLS coefficients for each outcome of interest.

Credit Constraints: If credit constraints motivate selection into cousin marriage, I would expect positive OLS coefficients on the timing of marriage. All else equal, individuals would delay entry into the marriage market until they have amassed sufficient funds to pay for marital transfers, increasing their age at marriage. Alternatively, if the timing of marriage stays the same, I would expect OLS to show a decrease the amount of money an individual pays in marital transfers. The estimated OLS effect on women's autonomy is ambiguous: credit constrained men would be willing to give potential spouses a larger share of total marital transfers (and hence, bargaining power), while credit constrained women would be willing to forgo autonomy within a marriage in order to make a match.

Consolidating Wealth: If cousin marriage is still used to reduce land fragmentation and consolidate wealth, the timing of engagement could speed up—possibly even before the age of majority; the length of an engagement may then increase proportionally, so that individuals marry as soon as they are legally able. OLS estimates could show that marital transfers are higher among consanguineous marriages, reflecting the wealth of the households that select into marriage.

Fear of Not Matching: Some individuals may anticipate difficulty in finding a match on the larger marriage market. They may expect their own quality to not be accurately perceived by potential spouses, or may anticipate experiencing higher search costs on the wider marriage market. These individuals might choose to marry a cousin—someone who they already know and might have a higher probability of successfully marrying—in lieu of conducting an extended search on the larger marriage market. In this case, all else equal, I would anticipate the timing of marriage to reflect the individual’s motivation to quickly finalize a marriage, resulting in lower ages of engagement and marriage in consanguineous unions when estimated by OLS. Individuals who are afraid of not matching with a spouse should also be willing to pay more in marital transfers to secure a spouse. At the same time, if the characteristics of the cousin they marry are worse than those of the unrelated spouse they would have married, the amount of marital transfers may be unchanged, while the characteristics of their spouse declined. The effects on marital transfers related to women’s bargaining power, as well as women’s autonomy would again be ambiguous; women would be willing to give up bargaining power and autonomy to secure a match, while men would be willing to give women more bargaining power and autonomy to secure a match.

Failure to Match: If individuals who were unable to find a spouse on the larger marriage market ultimately “settle” for a cousin, OLS coefficients would show a positive relationship between cousin marriage and the age at engagement and marriage. Such individuals might also be willing to pay more in marital transfers in order to match with a spouse. As with the fear of not matching, the relationship between cousin marriage and women’s empowerment is ambiguous.

1.5.2 Instrumental Variables

The previous section described some of the many possible unobserved characteristics that both influence individuals to choose to marry a cousin, and also affect the timing of marriage, the level and composition of marital transfers exchanged, and women's empowerment. A simple ordinary least squares regression would combine the true effect of cousin marriage with the effect of factors that determine selection into the institution. To isolate the impact of cousin marriage from the effect of omitted variables, I estimate results using instrumental variables. I leverage variation in family structure, interpreted through norms regarding marriage, to construct an instrument which is correlated with first cousin marriage, but which is not correlated with omitted variables or the error term.

The set of possible first cousins an individual can marry is the set of opposite-gender children of their parents' siblings. In Egypt, there are strong norms against men marrying older women ². As a result, the children of different sets of parents' siblings are suitable spouses for sons and daughters. Specifically, the *sons* of a parent's *older* siblings are more likely to be age-appropriate matches for daughters, while the *daughters* of a parent's *younger* siblings form the pool of most relevant matches for sons.

In Egypt, as in much of the world, wealth and agricultural land are passed down to children through inheritance. When women marry, they take assets to their husband's family, away from their natal family. Strategic matches between the children of brothers can reduce fragmentation of wealth and landholdings in future generations, and reduce the leakage of wealth from the family. In the Middle East, consanguineous marriage between brothers' children has historically been

²95% of the couples in my sample have an older husband.

termed *bint'amm* marriage [80]. Indeed, in some areas of Egypt, male cousins possess de facto rights to marry their father's brothers' daughters; before finalizing a girl's marriage to an outsider, permission must be granted from all male relatives who have such rights [21]. Given this context, I focus this paper on an individual's paternal cousins.³

My instrument interacts an individual's gender and their father's birth order (conditional on the father's set of siblings) to calculate the number of father's siblings who are most likely to have an age appropriate child for a son or daughter. For daughters, the instrument measures the number of her father's older siblings, while for sons, the instrument measures the number of his father's younger siblings. This variable, *# of Father's Relevant Siblings*, can be constructed as:

$$\mathbb{1}\{\text{Female}_{if}\} \times \underbrace{(\text{Father's Birth Order}_f - 1)}_{\# \text{ Father's Older Siblings}} + (1 - \mathbb{1}\{\text{Female}_{if}\}) \times \underbrace{(\text{Father's Siblings}_f + 1 - \text{Father's Birth Order})}_{\# \text{ Father's Younger Siblings}}$$

The first stage equation regresses an indicator variable which takes the value 1 if an individual i from father f married a first cousin, on the instruments and a set of controls:

$$\mathbb{1}\{\text{Married First Cousin}_{if}\} = \beta_1 + \beta_2 \times \# \text{ of Father's Relevant Siblings}_f + \mathbb{X}\mathbb{B} + \epsilon_f$$

If the instruments reflect the availability of age-appropriate cousins given the strong norm that women should marry older men, then β_2 should be positive: the more siblings an individual's father has who are likely to have age and gender appropriate children, the higher the probability of cousin marriage. Table 1.3 presents results that confirm this.

The second stage equation, which estimates the relationship between cousin marriage and

³I find that mother's birth order is only weakly correlated with cousin marriage conditional on father's birth order. I observe that 60% of cousin marriages are with cousins on the father's side.

the outcomes of interest, using the probability of cousin marriage predicted by the instrument, takes the form:

$$Y_{if} = \alpha_1 + \alpha_2 \times \widehat{\text{Married First Cousin}}_{if} + \mathbb{X}\mathbb{A} + u_f$$

where Y_{if} are outcomes related to the timing of marriage, marital transfers and women's autonomy. The effect of cousin marriage is given by α_2 .

1.5.2.1 Instrument Exogeneity

In order for my instrument to be valid, it must not directly impact any outcomes of interest (marital transfers, marriage timing, and women's empowerment). However, a father's birth order can be correlated with his education and earnings, which affect his ability to finance marital transfers for his children. In low income settings, higher birth order fathers have higher levels of education and earnings. Older children within a family leave school early to join the labor force, reducing their education and future earnings. Younger children benefit from the earnings of their older siblings, increasing their education and future earnings [35].

I address this possible violation of the exogeneity assumption in two ways. First, I test the correlation between my instrument, which relies on the interaction between an individual's gender and their father's birth order, and an individual's father's education and wealth. I present results in appendix table A1.3. I do not find a statistically significant association between the instrument and a father's wealth or education. This result is robust to adding controls.

Next, I directly test the relationship between an individual's father's wealth and the probability that his sons and daughters marry a cousin. I estimate this relationship on the sample of all

individuals in my sample, and present results in appendix table [A1.4](#). I find that father's wealth is negatively correlated with the probability of cousin marriage for both sons and daughters.

These checks suggest that father's wealth is associated with a lower probability of cousin marriage for both his sons and daughters, but that the instrument captures the impact of the availability of cousins, rather than any characteristics associated with father's wealth. I control for father's wealth in all specifications to ensure that my instruments only capture variation in the set of age-appropriate cousins which is orthogonal to father's wealth.

As a final robustness check, I examine whether a father's birth order or sibling composition is directly related to the number and composition of his children. I find that neither variable has an effect, and that this is robust to adding controls. I present results for these regressions in appendix table [A1.5](#).

1.5.2.2 Weak Instruments

My instrument does not satisfy the typical criteria for instrument strength; the regression F-statistic from the first stage is 5.4. The Kleibergen-Paap F-statistic is close to 3, depending on the outcome variable. This causes two problems: first, when instruments are weak, commonly used IV estimators such as 2SLS can fail to remove the bias in OLS. Second, traditional inference using t-ratios will have incorrect size.

Keane and Neal [[65](#)] provide a comprehensive explanation of the issues plaguing 2SLS in finite samples when instruments are weak. The authors explain that the estimator is biased towards OLS when the correlation between instruments and the endogenous variable is low. In small samples, the nonzero correlation between the instrument and the error term further ampli-

fies 2SLS bias. Alternative IV estimators, such as Limited Information Maximum Likelihood (LIML) may be less susceptible to this bias. However, LIML estimates are only valid under homoskedasticity. Since my endogenous variable is binary, it is necessarily heteroskedastic, and LIML is not a suitable method by which to estimate IV. I present point estimates from several different IV estimators: the unbiased estimator proposed by Andrews and Armstrong [11]; the Fuller IV estimator, setting α equal to 1; and finally, 2SLS.

Andrews and Armstrong [11] propose an IV estimator which requires a restriction on the sign of the first stage. This allows the authors to write the IV estimator as the product—rather than the ratio—of the expectation of two estimators, one of which is the unbiased estimator for the inverse of a normal mean. The resulting estimator is unbiased even when instruments are weak, and has moments, unlike 2SLS. The authors do not comment on statistical inference; t-ratio inference likely remains invalid for this estimator when instruments are weak.

The Fuller IV estimator is a k-class estimator which modifies the LIML estimator by $\frac{\alpha}{N}$, where α is a positive constant selected by the econometrician. Setting α equal to 1 is common in practice. The moments of the Fuller estimator exist, and the estimator appears to have better properties than 2SLS, both when instruments are weak as well as when they are strong [41, 46, 65].

Finally, I present results from 2SLS. While the properties of 2SLS are known to be poor when instruments are weak [65, 98], its ubiquity in applied work makes it a useful benchmark.

The second issue with weak instruments is the poor performance of t-tests. This is the subject of a large literature, which provides theoretical results and evidence from simulations [12, 45, 48, 65, 69, 103]. As recommended by Keane and Neal, Zivot et al., and Andrews et al., I use the Anderson Rubin (AR) test to judge the significance of each parameter estimate in lieu

of t ratio inference [10]. While there are several alternative tests which provide correct coverage in the case of weak instruments, in the case of a single instrument, the Anderson-Rubin test is uniformly most powerful. I use the STATA command **weakiv** created by Finlay et al. [39] to obtain the p-value from the Anderson-Rubin test. In an online appendix, I present confidence intervals calculated by inverting the AR test ⁴.

1.6 Results

I study three sets of socioeconomic outcomes affected by cousin marriage: (1) the timing of the match, (2) the level and allocation of marital transfers, and (3) women’s autonomy after marriage. For each outcome of interest, I present results instrumenting for cousin marriage as described in section 1.5.2. I include the same robust set of controls in each regression: fathers’ number of siblings, year of birth and an indicator for having any formal education; individuals’ characteristics (indicator variables for highest level of education, year of birth, and number of siblings), and fixed effects for region of residence. I also include indicators for the percentile of the asset index of the father’s household to account for natal household wealth.

In each table, I present point estimates using three different estimators: the Andrews & Armstrong unbiased estimator, the Fuller IV estimator (setting $\alpha = 1$), and 2SLS. While I present p-values from t-tests, these are incorrect. Instead, I assess statistical significance using the Anderson-Rubin test, and present associated p-values which are bolded for convenience. In addition to IV results, I report coefficients estimated using OLS, and the associated p-values. For each point estimate, which represents the effect of a 100 percentage point increase in the prob-

⁴Under homoskedasticity, it is possible to calculate a closed-form solution for the confidence intervals. However, grid search methods are required under heteroskedasticity of any form. I use an interval which is twice the length of the Wald confidence interval, and test 100 points within the interval [39].

ability of cousin marriage, I also calculate the corresponding impact of a one standard deviation increase in the probability of cousin marriage, (an increase from 0 to 0.373 percent).

1.6.1 Marriage Characteristics

Table 1.4 presents results for the timing of marriage. I look at four outcomes: the age of the individual in years at the time of engagement estimated separately by gender, the age difference between the individual and their spouse in years, and finally, the length of the engagement in months.

The first panel of column 1 results using the unbiased IV estimator proposed by [Andrews and Armstrong](#); this estimator finds that a one percentage point increase in the probability of cousin marriage would imply a 0.095 year (or one month) increase in men's age at marriage. The p-value from the Anderson-Rubin test is 0.06, suggesting that men whose family structure increases the likelihood that they have an age-appropriate female cousin exercise the option of consanguineous marriage at an older age than when they would have married had they not had a suitable cousin. The last row in the first panel shows that a one standard deviation increase in the probability of marrying a cousin is associated with a 3.55 year increase in the age of marriage for men.

The next two panels of column 1 present point estimates of the effect of cousin marriage on men's age at marriage using different IV estimators. The second panel shows point estimates from the Fuller estimator, setting the concentration parameter α equal to 1. Results are similar in magnitude to the Andrews & Armstrong unbiased estimator, and imply that a one percentage point increase in the probability of cousin marriage leads to a 0.079 year increase in men's age at

marriage. The third panel presents point estimates from 2SLS, which are larger in magnitude than either the Fuller or Andrews & Armstrong unbiased estimators, implying a 0.13 year increase in men's age at the time of marriage.

In contrast to IV results, OLS results in the penultimate panel of column 1 show that cousin marriage accelerates men's age at marriage by 0.009 years for every percentage point increase in the probability of cousin marriage. A one standard deviation increase in the probability of cousin marriage would decrease an individuals' age at marriage by about 4 months. OLS results measure the impact of cousin marriage on men who choose to marry a cousin as well as those who are motivated to do so due to variation in their family structure. OLS results have the opposite sign as IV results, which estimate effects for men motivated to marry a cousin due to their family structure. This stark contrast between OLS and IV results suggests that the unobserved factors that motivate men's selection into consanguineous marriages are negatively correlated with age. Inference solely with OLS regressions would have concluded that cousin marriage had the opposite effect and decreased men's age at marriage, when, in reality, it is more likely that the true effect is likely positive.

The first panel of the second column of table 1.4 presents IV results of the effect of cousin marriage on women's age at marriage, estimated using the Andrews & Armstrong unbiased estimator. A one percentage point increase in the probability of cousin marriage increases women's age at marriage by 0.027 years. However, the p-value from the Anderson-Rubin test is 0.472, and I cannot reject that the true effect is zero. The last line of the first panel of column 2 shows that a one standard deviation increase in the probability of cousin marriage should increase women's age at marriage by 1.01 years, though it is not possible to reject that the true effect is zero. The next two panels present IV point estimates from the Fuller estimator and 2SLS; the former im-

plies an increase of 0.026 years, while the latter shows that a one percentage point increase in the probability of cousin marriage increases women's age at marriage by 0.035 years. However, I cannot reject the null hypothesis that the true effect is zero, given that the Anderson-Rubin test p-value is 0.472.

OLS results indicate that women who marry cousins do so at a younger age than if they had married an unrelated spouse. For every percentage point increase in the probability of cousin marriage, women are 0.005 years younger when they marry cousins; an increase in the probability of cousin marriage by one standard deviation would decrease a woman's age at marriage by 0.18 years relative to if she had married a non-cousin. The OLS result is precisely estimated, unlike the IV; the contrast between the two suggests that selection into cousin marriage is negatively correlated with a women's age at marriage.

Column 3 of table 1.4 examines the impact of cousin marriage on the age difference between a man and his wife. In the first panel, I present IV results estimated using the Andrews & Armstrong unbiased estimator, which show that a one percentage point increase in the probability of cousin marriage is associated with a 0.1 year increase in the age gap between a man and his wife. The p-value from the Anderson-Rubin test is 0.044, which indicates that this finding is indeed statistically significant. The last row of the first panel scales the Andrews & Armstrong point estimate and shows that the effect of a one standard deviation increase in the probability of cousin marriage would increase the age gap between spouses by 3.76 years.

The following two panels present point estimates computed using different IV estimators. The Fuller estimator predicts a 0.087 year increase the age gap between men and their wives, while 2SLS predicts a larger increase in the age difference of 0.129 years between men and their wives if the probability of consanguineous marriage increases by one percentage point.

OLS results for the impact of cousin marriage on the age gap between men and their wives are imprecise, but paint a very different picture than the IV results. Every percentage point increase in the probability of cousin marriage is associated with a decrease in the age gap between men and their wives by 0.003 years. This point estimate is much smaller in magnitude than the IV estimates, and also has the opposite sign. Solely considering OLS estimates would have led us to the incorrect conclusion that cousin marriage decreased the age gap between men and their wives relative to marrying a non-relative. Selection into cousin marriage, which is responsible for differences between the OLS and IV estimates, appears negatively correlated with the age difference between men and their wives.

The penultimate column of table 1.4 looks at the difference in age between a woman and her husband. The first panel presents IV results estimated using the Andrews & Armstrong unbiased estimator, which shows that the age difference between a woman and her husband increases by 0.03 years. Since men are always older than their wives, this point estimate shows that the age gap becomes less negative, implying that women are closer in age to their husbands. However, the Anderson-Rubin p-value is 0.532, so I cannot reject the null hypothesis that the true effect is zero. Given this point estimate, a one standard deviation increase in the probability of cousin marriage should shrink the age gap between women and their husbands by 1.12 years.

The next panel of column 4 shows that when estimating effects using the Fuller estimator, a one percentage point increase in the probability of cousin marriage decreases the age gap between a woman and her husband by 0.029 years. Estimating the same IV using 2SLS would imply a 0.037 year decrease in the age gap between women and their spouses for every percentage point increase in the probability of cousin marriage. Again, these coefficients are not statistically significant given the large p-value associated with the Anderson-Rubin test.

The second to last panel of column 4 presents OLS results on the age gap between women and their husbands. OLS estimates suggest that a one percentage point increase in the probability of cousin marriage shrinks the age gap between women and their husbands by 0.002 years. However, this effect is not precisely estimated, and I cannot reject that the true effect is zero or even negative.

Finally, the last column of table 1.4 estimates the impact of cousin marriage on the length of the engagement period before marriage. The first panel estimates the IV using Andrews & Armstrong's unbiased estimator, which shows that a one percentage point increase in the probability of cousin marriage will increase the length of engagements by 0.128 months. The p-value of the Anderson-Rubin test is 0.371, which suggests that there is no statistically significant difference in the length of engagement between consanguineous and non consanguineous marriages. The last row of the first panel gives the effect of a one standard deviation increase in the probability of cousin marriage, based on the Andrews & Armstrong point estimate, which should increase the engagement length by 4.77 months.

Panel two presents the Fuller point estimate, which is similar to the Andrews & Armstrong unbiased estimator: a single percentage point increase in the probability of cousin marriage should increase the engagement period by 0.122 months. Estimates from 2SLS in the third panel are higher, and predict that a one percentage point increase in the probability of cousin marriage should lead to a 0.147 month increase in engagement periods before marriage. Note that none of these estimates are statistically significant; the p-value from the Anderson-Rubin test presented in the first panel is 0.371, so I cannot reject that the true effect is zero.

OLS results in the penultimate panel predict that a one percentage point increase in the probability of cousin marriage increases the length of an engagement between cousins by 0.036

months relative to if an individual had not married a cousin. A one standard deviation increase in the probability of cousin marriage is associated with a 1.34 month increase in the length of the engagement period. In this case, both the IV and OLS results have the same sign, though the IV results are not precisely estimated, and the OLS point estimates are smaller in magnitude. Together, these results suggest that there is some evidence that engagement periods are longer in marriages between cousins, compared to marriages between unrelated individuals.

Taken together, the IV results in table 1.4 show that the true effect of cousin marriage is to increase men's ages at marriage, which in turn increases the age difference between spouses. There do not appear to be any differences in women's age at marriage in consanguineous and non-consanguineous marriages, nor any differences in the length of engagements. These results are consistent with a pattern where men who have an age and gender appropriate cousin delay marriage, and ultimately exercise the option to marry a cousin. This delay could be consistent with several of the motivations discussed in section 1.5.1.1: credit constraints may cause delayed entry into the marriage market if individuals take time to amass wealth, or alternatively, men marry cousins after conducting a lengthy and fruitless search for a wife on the wider marriage market. At the same time, compliers may have delayed marriage precisely because they were aware of an appropriate cousin they could marry. If not for the existence of that cousin, they may have married an unrelated partner earlier.

The pattern of selection evident in the OLS results shows that unobserved characteristics which motivate individuals to marry a cousin appear negatively correlated with age of marriage and the age gap between the individual and their spouse. Of the motivations discussed in section 1.5.1.1, the results in table 1.4 are consistent with either protecting property, or fear of not matching on the wider marriage market. Both OLS and IV results suggest that engagement pe-

riods could be longer in cousin marriage, which could be consistent with credit constraints or protecting property as motivations for cousin marriage.

1.6.2 Marital Transfers

The next set of outcomes I consider are marital transfers, which I present in table 1.5. These transfers are both a significant economic transfer to the new couple, and a source of bargaining power for brides. I first consider transfers that are given wholly to the bride: the dower, deferred dower (mo'akhar), and shabka. I also present results on the value of the ayma, which states the value of all marital transfers which become the property of the bride upon dissolution of the marriage. Next, in table 1.6, I look at transfers which are given jointly to the bride and groom for their use: the bridal trousseau and household goods (gihaz), and the large household goods (afsh). I also consider the value of housing given to the couple, as well as the amount spent on celebrations. The data on nominal expenditures was top-censored at E£99,999. I convert all values to 2010 E£. For ease of exposition, I divide all transfer amounts by 1,000; the coefficients represent effects in thousands of pounds. Point estimates show the effect of a 100 percentage point increase in the probability of consanguineous marriage; to obtain the impact of a one percentage point increase in the probability of cousin marriage, I divide coefficients by 100.

In columns 1 and 2 of table 1.5, I examine the impact of cousin marriage on the dower, a lump sum cash transfer given from the groom and his family to the bride herself. Column 1 considers the amount of dower given as the outcome, and column 2 looks at a binary variable which takes the value of one if a woman received a nonzero amount of dower. The first panel of table 1.5 presents IV results estimated using the Andrews & Armstrong unbiased estimator.

A one percentage point increase in the probability of cousin marriage yields a £87 increase in the value of dower exchanged in the marriage, compared to the amount that would have been exchanged between unrelated spouses. The p-value of 0.56 from the Anderson-Rubin test indicates that I cannot reject that the effect of cousin marriage on the amount of dower a woman receives is zero. Similarly, in panel one of column 2, the Andrews & Armstrong unbiased estimator shows that the probability a dower is exchanged at marriage rises by 0.008 percentage points with every percentage point increase in the probability of cousin marriage, compared to the probability of exchanging a dower when spouses are not first cousins. The corresponding Anderson-Rubin p-value of 0.905 indicates that this result, too, is not statistically significant. Together, these results suggest that value of dower exchanged is not different in consanguineous and exsanguineous marriages. The last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage: this should increase the amount of dower a woman receives by £3,240, and make her 3 percentage points more likely to receive a dower. However, as the p-values from the Anderson-Rubin test show, these effects are not precisely estimated, and I cannot reject that the true effect is zero.

In the second panel, I present estimates using the Fuller estimator; a one percentage point increase in the probability of cousin marriage increases dower by £74.67, and increases the probability that a woman receives a dower by 0.0006 percentage points. Estimates using 2SLS show that a one percentage point increase in the probability of cousin marriage increases the dower a woman receives by £120, and raises the probability she will receive any dower by 0.001 percentage points. However, note that these estimates are not precise. Given the large p-values from the Anderson-Rubin test, I cannot reject that the effect is actually zero.

OLS estimates indicate a much smaller impact on the amount of dower given to the bride,

as well as the probability the bride receives a dower. A single percentage point increase in the probability of cousin marriage decreases the amount of dower given to a woman by E£2.03, and decreases the probability she receives a dower by 0.0002 percentage points, relative to if she had married a non-relative. These estimates are imprecisely estimated, and I cannot reject that the coefficient could be zero or positive. The contrast between IV results and OLS results suggests that unobserved factors that prompt selection into cousin marriage are negatively correlated with the amount of dower given to the bride, and the probability she receives any dower.

In column 3 of table 1.5, I look at the impact of cousin marriage on the value of shabka, or gold jewelry, given to the bride at her engagement. Results estimated using the Andrews & Armstrong unbiased estimator show that the value of shabka increases by E£285 for every percentage point increase in the probability of cousin marriage, compared to if a woman had not married a cousin. The Anderson-Rubin p-value is 0.035, indicating that this result is statistically significant. The last row of panel 1 shows that a one standard deviation increase in the probability of cousin marriage increases the amount of shabka by E£10,620. In the second panel, I estimate IV results using the Fuller estimator I find an increase in the value of shabka of E£263 for every percentage point increase in the probability of cousin marriage. Finally, 2SLS estimates show that shabka increases by E£349 for every percentage point increase in the probability of cousin marriage.

In contrast, OLS results in the penultimate panel imply that a bride would receive E£1.60 less shabka for every percentage point increase in the probability in cousin marriage. A one standard deviation increase in the probability of cousin marriage implies a decrease of E£60 in the value of shabka, when estimated by OLS. Relying on OLS estimates would have caused us to incorrectly conclude that cousin marriage causes a decrease in the amount of shabka in

a marriage; in reality, the unobserved factors which motivate selection appear to be negatively correlated with the value of shabka.

The penultimate column of table 1.5 looks at the amount of mo'akhar, or deferred dower, promised to women at the time of marriage. Point estimates from the Andrews & Armstrong unbiased estimator show that the amount of mo'akhar promised to women increases by E£363 for every percentage point increase in the probability of cousin marriage. The p-value from the Anderson-Rubin test is 0.04, and I can reject the null hypothesis that the effect of cousin marriage is zero. A one standard deviation increase in the probability of cousin marriage increases the amount of mo'akhar promised by E£13,540.

Fuller point estimates, which I present in the second panel, are similar, showing a E£331 increase, while point estimates from 2SLS, presented in the third panel predict a E£450 increase in the amount of mo'akhar promised for every percentage point increase in the probability of cousin marriage.

However, results estimated by OLS in the fourth panel of column 4 show that a one percentage point increase in the probability of cousin marriage is associated with a decrease of E£5.47 in the value of mo'akhar. A one standard deviation decrease in the probability an individual marries a cousin would imply a decrease of E£200 in the mo'akhar promised to the bride. The difference between IV and OLS results shows the extent to which selection affects transfers given in cousin marriages compared to non-cousin marriage: factors associated with an increase in the probability of cousin marriage are negatively correlated with the amount of deferred dower promised to the bride.

The final column of table 1.5 looks at the effect of cousin marriage on the ayma, or the sum of all marital transfers aside from housing, which are property of the bride upon dissolution of

the marriage. Estimating results using the Andrews & Armstrong unbiased estimator shows that the value of ayma increases by E£806 for every percentage point increase in the probability of cousin marriage; the p-value of 0.068 shows that I can reject that the effect is zero with almost 95% certainty. A one standard deviation increase in the probability of cousin marriage increases the value of the ayma by E£30,050.

The next panel shows that results estimated using the Fuller estimator produce a slightly smaller increase of E£726 for the same increase in the probability of cousin marriage, while 2SLS produces the largest estimate of E£1,036 per percentage point increase in the probability of cousin marriage. Note that the p-values from t-ratio inference are incorrect; these results are statistically significant at the 10 percent level as per the Anderson-Rubin p-values in the first panel.

In contrast to IV results, OLS gives the opposite prediction, and shows that the ayma decreases by E£13 for every percentage point increase in cousin marriage, though this coefficient is not precisely estimated. The contrast between OLS and IV estimates imply that the unobserved factors that motivate selection into cousin marriage are negatively correlated with the amount of ayma given in a marriage.

Together, these results show that when individuals are exogenously motivated to marry a cousin, the value of shabka, mo'akhar, and ayma are higher than in marriages between two unrelated individuals. Contrasting IV estimates with OLS coefficients shows that factors that influence selection are negatively correlated with the probability of a woman receiving a dower and the value of that dower, and are also negatively correlated with other marital transfers that give women wealth and bargaining power. Given that the mo'akhar requires no expenditure at the time of marriage, increasing its value is a relatively costless way to increase women's bargaining

power without incurring additional expenses at the time of marriage. The larger increases of the mo'akhar and ayma (of which the mo'akhar is a component) compared to the shabka reflect this.

In table 1.6, I consider the set of marital transfers that are initially given jointly to the couple: the gihaz, or bridal trousseau and household goods, the afsh, or set of large household goods, the value of housing given to the couple, and the amount spent on celebrations. Of these transfers, the gihaz and afsh form part of the ayma, along with the shabka, dower, and mo'akhar. The bride typically does not retain access to the housing provided as part of marital transfers if the marriage dissolves, and is expected to move back to her natal home.

The first column of table 1.6 examines the impact of cousin marriage on the value of the gihaz. Point estimates from the Andrews & Armstrong unbiased estimator suggest that the value of the gihaz in consanguineous marriages increases by E£225 for every percentage point increase in the probability of cousin marriage. However, this result is imprecisely estimated, which is apparent in the large p-value from the Anderson-Rubin test, and I cannot reject the null hypothesis that the true effect is zero. Point estimates using the Fuller estimator are smaller, showing a E£187 increase, while 2SLS shows a E£307 increase in the value of gihaz. Again, note that none of these point estimates are statistically significant, given the large p-value of the Anderson-Rubin test.

In contrast, the OLS results imply that a one percentage point increase in the probability of cousin marriage is associated with a E£10 decrease in the value of the gihaz. This estimate reflects the effect of selection: the unobserved factors which increase the likelihood an individual marries a cousin appear negatively correlated with the amount of gihaz exchanged at marriage. However, this result is not precisely estimated.

Column 2 of table 1.6 presents the effect of cousin marriage on the value of the afsh, or set of large household furnishings and appliances. The Andrews & Armstrong unbiased estimator

shows that the value of afsh increases by £375 for every percentage point increase in the probability of cousin marriage, while the Fuller point estimate shows an increase of £339. 2SLS predicts the largest increase of £466 for every percentage point increase in the probability of cousin marriage. Given the p-value of 0.227 from the Anderson-Rubin test, I cannot reject that cousin marriage has no effect on the value of afsh.

OLS results estimating the effect of cousin marriage on the value of the afsh have the same sign as IV results, but are significantly smaller in value: a one percentage point increase in the probability that an individual marries a cousin is associated with a £1.39 increase in the value of the afsh relative to the value which would have been exchanged had the individual had not married a cousin. However, neither the OLS nor IV results are precisely estimated. While point estimates indicate there may be an increase in the value of the afsh in cousin marriages, I cannot reject that cousin marriage has no impact on the value of this transfer.

The penultimate column of table 1.6 looks at the effect of cousin marriage on the value of housing given to the couple. Point estimates of the value of housing given to the couple are negative. Estimates using the Andrews & Armstrong unbiased estimator show the value of housing decreases by £169 for every percentage point increase in the probability of cousin marriage. A one standard deviation increase in the probability of cousin marriage should decrease the value of housing provided to the couple by £6,300. However, the p-value of the Anderson-Rubin test does not allow us to reject that the true effect is zero. The Fuller estimator is of a similar magnitude, indicating a decrease of £157 for the same change in the probability of cousin marriage. Finally, 2SLS presents the largest effects, showing a decrease of £204 for every percentage point increase in the probability of cousin marriage. Nevertheless, none of these point estimates are statistically significant, as the p-value from the Anderson-Rubin test is

0.665.

In contrast, OLS estimates find that a one percentage point increase in the probability of consanguineous marriage is associated with a £14 increase in the amount spent on housing. The OLS estimate has the opposite sign as the IV estimate, suggesting that the selection into cousin marriage is positively correlated with expenditures on housing. However, the OLS estimate is imprecisely estimated, so I cannot reject that cousin marriage has no effect on the value of housing provided to couples in consanguineous unions.

Finally, I test whether there are differences in the amount that families spend on marriage celebrations between consanguineous and non-consanguineous marriages. The Andrews & Armstrong unbiased estimator shows that the amount spent on celebrations increases by £211 for every percentage point increase in the probability of cousin marriage. This point estimate implies that a one standard deviation increase in the probability of cousin marriage may increase the amount spent on marriage celebrations by £7,870. However, I cannot rule out that cousin marriage leaves expenditures unchanged, as the p-value from the Anderson-Rubin test is 0.185. The Fuller estimator shows a similar result to the Andrews & Armstrong unbiased estimator, and predicts a £189 increase in the amount spent on celebrations for every percentage point increase in the probability of cousin marriage, while the 2SLS estimator gives a larger point estimate of £269. None of these estimators are statistically significant, given that the Anderson-Rubin p-value is 0.185.

OLS estimates are opposite in sign to IV estimates, suggesting an increase of £2.82 in the amount spent on celebrations for every percentage point increase in the probability that an individual marries a cousin. Though both the IV and OLS estimates predict an increase in the amount spent on celebrations, I cannot reject that the true effect is equal to zero, as neither

coefficient is precisely estimated.

Overall, IV estimates show that individuals in consanguineous marriages have higher expenditures on shabka, mo'akhar, ayma, but no differential expenditures on gihaz and afsh, dower, housing and celebrations. Contrasting these results with OLS show that the unobserved factors that motivate selection into cousin marriage are negatively correlated with expenditures on shabka, ayma, mo'akhar, but potentially positively correlated with expenditures on housing. Combined with the results showing that men marry at older ages, these results are consistent with a scenario where men who have the option to marry a cousin delay marriage—possibly because they are unsuccessful on the wider marriage market, or are waiting to save and finance marital transfers—and then marry female cousins who are younger than them, but have to compensate their wives for their advanced age, which is a disamenity.

1.6.3 Women's Autonomy & Welfare

The final set of outcomes I consider are related to women's autonomy. Jacoby and Mansuri [57] find that decreasing social distance between in-law households can decrease the incidence of intimate partner violence in Pakistan. I lack data on intimate partner violence, but I study women's physical autonomy and decisionmaking ability after marriage, which are other meaningful measures of women's welfare. I create two separate indices to measure women's decision-making abilities and physical mobility. I also consider whether the woman in each marriage can control household money, and whether or not she lives in a joint household.

The first panel of the first column of table 1.7 presents point estimates using the Andrews & Armstrong unbiased estimator; these show that a one percentage point increase in the probability

of cousin marriage increases the value of the decisionmaking index by 0.035 standard deviations. This implies that a one standard deviation increase in the probability of marriage should increase the decisionmaking index by 1.29 standard deviations. I cannot rule out that cousin marriage has no impact on women's ability to make decisions, as the p-value from the Anderson-Rubin test is 0.148. The Fuller estimator presented in panel 2 is similar in magnitude to the Andrews & Armstrong estimator, also showing an increase of 0.032 standard deviations per percentage point increase in the probability of cousin marriage. 2SLS produces a larger point estimate, of 0.042 standard deviations. However, none of these results are estimated with precision.

OLS results are less precisely estimated, and show that a percentage point increase in an individual's probability of cousin marriage decreases the decisionmaking index by 0.0005 standard deviations. The OLS and IV results have the same sign, but neither is precisely estimated; while these results may indicate that cousin marriages improve women's decisionmaking abilities, I cannot reject that cousin marriage has no impact on this margin.

In the next column, I look at how cousin marriage might change the physical mobility of women, measured by the number of places they are able to travel without supervision. Point estimates on an index measuring women's physical mobility are negative and smaller in magnitude than effects on a similar index measuring women's decisionmaking abilities. The Andrews & Armstrong unbiased estimator shows that a one percentage point increase in the probability of cousin marriage decreases the value of the mobility index by 0.016 standard deviations. This implies that a one standard deviation increase in the probability of cousin marriage decreases the value of the index by 0.59 standard deviations, and that women are less able to travel without supervision in consanguineous marriages. The Fuller estimator shows a decrease of 0.015 standard deviations, while 2SLS shows a decrease of 0.019 standard deviations. While point es-

timates may be negative, I cannot rule out that the effect of cousin marriage on women's physical mobility is zero, given that the p-value of the Anderson-Rubin test is 0.387.

OLS results, in contrast, imply that an equivalent increase in the probability an individual marries a cousin is associated with a 0.0007 decrease in the mobility index: women have less autonomy over their physical mobility in cousin marriages relative to their physical mobility otherwise. The OLS and IV results have the same sign, which implies that cousin marriage may negatively impact women's physical mobility, though neither estimate is statistically significant.

In column 3 of table 1.7, I explore the relationship between consanguineous marriage and women's ability to use household money. Point estimates suggest the effect is negative, though imprecisely estimated. The Andrews & Armstrong unbiased estimator shows that the probability a woman can use household money falls by 0.003 percentage points for every percentage point increase in the probability of cousin marriage; this implies a 0.12 percentage point decrease in the probability of using household money given a one standard deviation increase in the probability of cousin marriage. The Fuller estimator predicts the same effect as the Andrews & Armstrong unbiased estimator, while 2SLS finds a slightly larger decrease of 0.004 percentage points for every percentage point increase in the probability of cousin marriage. However, no point estimate is precisely estimated: the p-value from the Anderson-Rubin test is 0.579, so I cannot reject that the true effect is zero.

OLS results are also negative, but much smaller in absolute value. A one percentage point increase in the probability that an individual marries a cousin is associated with a 0.0002 percentage point decrease in the probability that the woman in the marriage can use household money. This suggests that cousin marriage may indeed be associated with a reduction in the probability that a woman can use household money, but again, I cannot rule out that the effect is zero.

In the final column of table 1.7, I assess whether cousin marriage affects the living arrangements of individuals after marriage. Point estimates are very small, though again, imprecisely estimated. The Andrews & Armstrong unbiased estimator shows that the probability of living in a joint household falls by 0.0008 percentage points for every percentage point increase in the probability of cousin marriage; in turn, this implies that a one standard deviation increase in the probability of cousin marriage decreases the chance of living in a joint household by 3 percentage points. The Fuller coefficient shows a decrease of 0.0006 percentage points, while 2SLS predicts a decrease of 0.001 percentage points. However, the p-value from the Anderson-Rubin test is 0.87, so I cannot reject that the true effect is zero.

OLS results show a small, precisely estimated increase of 0.0009 percentage points for every percentage point increase in the probability of cousin marriage. A one standard deviation increase in the probability of cousin marriage implies a 3 percentage point increase in the probability that an individual lives in a joint household. Compared to IV results, the OLS results indicate that selection is positively correlated with living in a joint household.

While IV point estimates suggest that the causal impact of cousin marriage is to increase women's decisionmaking ability but decrease her physical mobility, use of household money, and probability of living in a joint household. However, none of these results are precisely estimated, and I cannot rule out that the true effect is zero. Contrasting IV results with OLS shows the impact of selection into cousin marriage: that selection may be negatively correlated with women's decisionmaking ability, but positively correlated with the probability a couple lives in a joint household.

1.6.4 Possible Mechanisms

Causal estimates of the impact of cousin marriage show that men marry at an older age than if they had married a non-relative, and that the age differences between men and their wives increase in consanguineous marriages. Marital transfers that give women bargaining power—the shabka, ayma, and mo'akhar—are higher when individuals are exogenously motivated to marry a cousin. However, I do not find that there are statistically significant impacts of cousin marriage on women's decisionmaking abilities, physical mobility, use of household money, or likelihood of living in a joint household. These results suggest that cousin marriage may be mutually beneficial to a subset of individuals: men who delayed marriage find a spouse, and women who marry older cousins are compensated for this disamenity through an increase in bargaining power.

There are multiple explanations for why men with an age and gender appropriate cousin in their families delay marriage. It may be due to their attributes not being valued on the wider marriage market, facing higher search costs, or intentionally delaying entry into the marriage market in order to amass human capital or sufficient funds to finance marital transfers. The effect of exogenous family structure, which is determined early on, may also affect individuals' marriage market decisions: they may delay marriage precisely because they know that they have a cousin as a fallback option.

In turn, the female cousins these men marry are compensated for marrying a spouse with either real or perceived poorer attributes, including their spouse's increased age. They are able to obtain more in marital transfers that are directly related to their bargaining power than if they had married an unrelated spouse. Like their male cousins, women may also make choices knowing they have a potential consanguineous spouse determined by their exogenous family structure.

They may choose to forgo investments in human capital and marry a cousin instead of competing on the wider marriage market.

Contrasting causal impacts with OLS results shows the type of selection at play. Unobserved factors which are associated with an increased probability of marrying a cousin are negatively correlated with age at marriage, and age differences between individuals and their spouses. These factors are also negatively correlated with the amount of marital transfers given to women. Of the motivations discussed in section [1.5.1.1](#), one that is consistent with this pattern of results is that individuals who fear not matching on the wider marriage market rush to marry a cousin. Not only should the ages of individuals be lower since they rush to finalize a match, the amount of transfers are lower, either due to giving up value of transfers in order to secure a match earlier, or due to forgoing search on the wider marriage market due to credit constraints, as unrelated potential spouses demand larger transfers than socially proximate relatives. Lastly, selection might occur to concentrate family property: I see matches finalized at an earlier age, and lower levels of women's empowerment. Families may choose to give lower levels of lump-sum transfers at the time of marriage, and instead wait to transfer wealth through inheritance at a later time.

My IV results may be affected by strategic behavior on the part of households who try to maximize welfare over all of their children's marriages, rather than maximizing the welfare of the individual who is to be married. Parents may accept marital contracts that may not be optimal for a particular child, but which may be optimal for the set of children overall, or for a subset of children they have a stronger incentive to match well (ie sons). If I observe outcomes for both siblings in my dataset, correlation between their marital matches could generate bias in how my coefficients are estimated. At best, this would attenuate the results to zero, and at worst, it could bias my coefficients. I estimate results restricting the sample to one child per father I observe

in the data. I do this in three ways: keeping the youngest child, the oldest child, or randomly selecting one child. I do not find meaningful differences in the point estimates, though precision further declines. These results are available on request.

Importantly, heterogeneous treatment effects on various dimensions may be a contributing factor to the imprecisely estimated IV results. For example, men and women have different outside options to marrying a cousin; that is, the women men would have married if they had not married a cousin, and the men women would have married had they not married a cousin, are not necessarily similar, or even symmetric. Certain treatment effects, especially those related to marital transfers, could be positive for men, or negative for women. The level of marital transfers men would have had to pay to an unrelated spouse may be lower than the level of transfers that women would have received from an unrelated spouse. The IV treatment effect would aggregate these two potentially precisely estimated effects into an imprecisely estimated coefficient. Ideally, I would be able to allow the effect of cousin marriage to vary by gender, or any other dimension of heterogeneity. However, I lack the statistical power to estimate this due to the small sample size. I hope to be able to test this in a larger sample in the future.

1.7 Conclusion

Individuals in consanguineous marriages may have different outcomes than those who marry outside of their extended family, but these differences might arise solely from unobserved characteristics which drive selection. When I use exogenous variation in family structure to instrument for the probability of marrying a cousin, I am able to abstract from characteristics that are correlated with both selection into cousin marriage, and the outcomes of consanguineous mar-

riages. I find that men exogenously motivated to marry cousins do so at an older age, and have a larger age gap with their wives. Women in consanguineous marriages receive larger marital transfers which contribute to their bargaining power within the marriage. I find no differences in women's autonomy or physical mobility after marriage.

Contrasting causal IV estimates with OLS results shows that unobserved factors that increase the probability that an individual chooses to marry a cousin exert significant influence on the results. This pattern of selection is consistent with several underlying motivations for consanguineous marriage: credit constraints, the fear of not matching on the marriage market, or the desire to concentrate family property.

Obtaining causal estimates of the effect of cousin marriage on socioeconomic outcomes, when selection plays a huge role in the difference in average outcomes I observe in consanguineous and non-consanguineous marriage is crucial to identifying and alleviating the constraints that motivate this practice, especially give that cousin marriage can be beneficial to women in the absence of selection.

1.8 Tables & Figures

Table 1.1: Hypothesized OLS Effects

	Credit Constraints		Fear of Not Matching		Failed to Match		Protect Property	
	Groom	Bride	Groom	Bride	Groom	Bride	Groom	Bride
Men: Age at Engagement	+	+	–	–	+	+	–	–
Women: Age at Engagement	?	?	?	?	?	?	–	–
Engagement Length	+	+	–	–	–	–	+	+
Total Marital Transfers	–	–	?	?	?	?	+	+
Own Family Marital Transfers Paid	–	–	+	+	+	+	+	+
Spouse Family Marital Transfers Paid	?	?	–	–	–	–	?	?
Amount Marital Transfers Related To Bargaining Power	+	–	+	–	+	–	?	?
Share Marital Transfers Paid	?	?	+	+	+	+	?	?
Women's Empowerment	–	–	+	–	+	–	–	+

Table 1.2: Summary Statistics by Marriage Type

	Exsanguineous (N=5,179)	Consanguineous (N=1,038)	Exsanguineous - Consanguineous (Total N = 6,217)
	Mean (SD)	Mean (SD)	Difference (P Value)
Female	0.52 (0.500)	0.45 (0.50)	0.07 (0.00)
Men: Age at Marriage	26.24 (3.952)	25.02 (3.80)	1.22 (0.00)
Women: Age at Marriage	21.45 (4.062)	20.60 (3.63)	0.85 (0.00)
Months btwn engagement marriage	12.25 (10.627)	15.10 (14.18)	-2.85 (0.00)
Men: Age Difference with Wife	5.09 (3.924)	5.11 (3.72)	-0.02 (0.92)
Women: Age Difference with Husband	-5.63 (4.095)	-5.39 (3.91)	-0.24 (0.29)
Father's birth order	3.04 (2.088)	3.06 (2.07)	-0.02 (0.73)
Father's no. of brothers	3.05 (2.076)	3.32 (2.14)	-0.27 (0.00)
Father's no. of sisters	2.72 (1.922)	2.76 (2.04)	-0.04 (0.58)
Urban	0.40 (0.489)	0.28 (0.45)	0.11 (0.00)
Total amt spent on wedding 2010 LE	93,340.82 (73,121.458)	87,388.02 (71,947.28)	5,952.81 (0.02)
Amount paid by own family 2010 LE	45,436.17 (49,213.005)	46,672.84 (51,985.19)	-1,236.67 (0.47)
Amount paid by spouse family 2010 LE	47,904.68 (49,368.718)	40,715.20 (46,467.61)	7,189.48 (0.00)
Amt dower to bride 2010 LE	2,699.71 (10,524.228)	2,869.01 (10,528.32)	-169.30 (0.66)
Amt of jewelry given to bride 2010 LE	8,638.59 (9,659.564)	8,539.98 (9,956.98)	98.61 (0.77)
Amt future transfer to bride 2010 LE	7,156.30 (12,572.794)	6,254.91 (11,852.66)	901.39 (0.05)
Amt of bridal trousseau 2010 LE	19,617.41 (20,839.703)	16,354.35 (19,361.23)	3,263.07 (0.00)
Amt of hh goods 2010 LE	28,066.94 (24,536.508)	24,635.07 (24,589.69)	3,431.87 (0.00)
Empowerment Index	0.17 (2.096)	-0.06 (2.02)	0.23 (0.00)
Decisionmaking Index	0.03 (2.035)	-0.14 (1.93)	0.18 (0.01)
Mobility Index	0.31 (1.482)	0.20 (1.55)	0.11 (0.03)

Summary statistics of sample of 6,127 individuals. Rate of consanguineous marriage in sample is 16.7%. Annual household income in 2010 was E£25,353. In 2010, 1 E£≈ 0.18 USD

Table 1.3: First Stage: Father's Birth Order on Probability Married First Cousin

	(1)	(2)	(3)	(4)
Number of relevant father siblings	0.00418 (0.078)*	0.00419 (0.078)*	0.00382 (0.107)	0.00457 (0.054)*
Constant	0.155 (0.000)***	-1.887 (0.080)*	-3.714 (0.011)**	-2.886 (0.048)**
Gender	Yes	Yes	Yes	Yes
Father Controls	Yes	Yes	Yes	Yes
Siblings			Yes	Yes
Geography				Yes
Year of Birth			Yes	Yes
Education			Yes	Yes
Wealth				Yes
F Statistic	8.729	13.440	11.041	5.599
Obs	6,205	6,061	5,955	5,955

OLS regressions, clustering standard errors at the fathers' household level. P-values presented in parentheses below point estimates. Rate of consanguineous marriage in sample is 16.7%.

Table 1.4: Marriage Timing

	Men Age Marriage (1)	Women Age Marriage (2)	Men: Years Older Than Wife (3)	Women: Years Older Than Husband (4)	Months Engaged (5)
Andrews & Armstrong Coefficient	9.523 (0.435) (0.060)	2.706 (0.619) (0.472)	10.085 (0.337) (0.044)	2.991 (0.631) (0.532)	12.792 (0.463) (0.371)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	3.55	1.01	3.76	1.12	4.77
Fuller Coefficient	7.909 (0.181)	2.601 (0.519)	8.707 (0.130)	2.893 (0.541)	12.248 (0.355)
2SLS Coefficient	13.031 (0.285)	3.519 (0.518)	12.948 (0.217)	3.673 (0.555)	14.723 (0.398)
OLS Coefficient	-0.893 (0.000)	-0.496 (0.002)	-0.300 (0.081)	0.174 (0.447)	3.606 (0.000)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.330	-0.180	-0.110	0.060	1.340
N	2,959	2,947	2,824	2,350	5,914
Kleibergen-Paap F	1.848	3.194	2.274	3.301	3.609
Exsanguineous Mean	26.240	21.450	5.090	-5.630	12.250

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence.

Table 1.5: Marital Transfers to Women

	Dower (1)	Any Dower (2)	Shabka (3)	Mo'akhar (4)	Ayma (5)
Andrews & Armstrong Coefficient	8.697 (0.701) (0.560)	0.080 (0.932) (0.905)	28.461 (0.268) (0.035)	36.310 (0.287) (0.040)	80.558 (0.361) (0.068)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	3.24	0.03	10.62	13.54	30.05
Fuller Coefficient	7.467 (0.580)	0.063 (0.914)	26.253 (0.116)	33.081 (0.123)	72.622 (0.164)
2SLS Coefficient	11.996 (0.597)	0.111 (0.905)	34.924 (0.174)	45.015 (0.187)	103.556 (0.241)
OLS Coefficient	-0.203 (0.607)	-0.019 (0.263)	-0.160 (0.652)	-0.547 (0.234)	-1.329 (0.287)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.080	-0.010	-0.060	-0.200	-0.500
N	5,253	5,322	5,661	5,243	5,095
Kleibergen-Paap F	1.724	1.778	3.131	2.862	2.370
Exsanguineous Mean	2.700	0.310	8.640	7.160	46.210

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence. Transfer amounts divided by 1000; coefficients represent effects in thousands of E£.

Table 1.6: Marital Transfers to Couple

	Gihaz (1)	Afsh (2)	House (3)	Celebration (4)
Andrews & Armstrong Coefficient	22.463 (0.635) (0.453)	37.472 (0.428) (0.227)	-16.885 (0.727) (0.665)	21.086 (0.428) (0.185)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	8.38	13.98	-6.30	7.87
Fuller Coefficient	18.729 (0.488)	33.872 (0.281)	-15.656 (0.676)	18.857 (0.250)
2SLS Coefficient	30.742 (0.516)	46.625 (0.324)	-20.377 (0.673)	26.926 (0.311)
OLS Coefficient	-1.006 (0.164)	0.139 (0.868)	1.435 (0.257)	0.282 (0.539)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.380	0.050	0.540	0.110
N	4,931	5,472	5,253	5,178
Kleibergen-Paap F	1.660	2.736	3.686	2.335
Exsanguineous Mean	19.620	28.070	28.200	7.710

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence. Transfer amounts divided by 1000; coefficients represent effects in thousands of EF.

Table 1.7: Women's Autonomy

	Decisionmaking Index (1)	Mobility Index (2)	Use HH Money (3)	Joint HH (4)
Andrews & Armstrong Coefficient	3.465 (0.337) (0.148)	-1.591 (0.498) (0.387)	-0.312 (0.655) (0.579)	-0.081 (0.899) (0.870)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	1.29	-0.59	-0.12	-0.03
Fuller Coefficient	3.240 (0.215)	-1.489 (0.406)	-0.297 (0.584)	-0.062 (0.901)
2SLS Coefficient	4.162 (0.249)	-1.877 (0.424)	-0.373 (0.594)	-0.103 (0.871)
OLS Coefficient	-0.053 (0.432)	-0.066 (0.224)	-0.019 (0.251)	0.088 (0.000)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.020	-0.020	-0.010	0.030
N	5,928	5,949	5,949	5,955
Kleibergen-Paap F	3.672	3.787	3.787	3.718
Exsanguineous Mean	0.030	0.310	0.670	0.280

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence.

Chapter 2: Jobs and Intimate Partner Violence: Evidence from Bangladesh

2.1 Introduction

Economists have extensively documented the long-term welfare improving impacts of women's labor force participation. Increasing female labor force participation broadly increases economic growth [38], and improving labor market opportunities for women is an explicit target of the UN Sustainable Development Goals [1]. Women who take advantage of employment opportunities may increase household income, improve outcomes for their children, and make more decisions within the household [50]. Even the promise of suitable labor market opportunities for women is associated with increases in girls' education, and women's age at first birth. However, in the short run, women's entry in the labor market may cause frictions within the household. While not universal, there is a well-documented positive association between women's labor force participation and their experiences of intimate partner violence. This relationship is present in both low and high income settings across the globe. Strong adherence to gender norms, acceptability of violence, and women's inability to access divorce, are factors which increase the likelihood that women are subject to intimate partner violence when they enter the labor market [13, 29, 68]. Identifying the causal link between female labor force participation and intimate partner violence is difficult: women's entry into the labor force is correlated with, and often endogenous to, their characteristics and the characteristics of their households. Identifying whether violence is a mo-

tivation for, or a response to, female labor force participation is difficult, especially since there may be additional unobserved factors at play.

Giné et al. [43] run an experiment in Bangladesh which aims to increase women's labor force participation and use of mobile money technology. The authors work with owners of existing mobile money businesses who want to expand their operations by hiring an additional employee. Each business owner enters two lotteries: the first determines whether the employer receives a wage subsidy to hire an additional worker, and the second identifies which employee the business owner must hire from the pool of two men and two women he nominated. I leverage the second lottery to study the impact of receiving a job offer on women's experiences of intimate partner violence. I survey all potential employees nominated by business owners, as well as their spouses, before the randomization takes place, as well as 6 months after they begin work. I collect data on a variety of topics including women's experiences of intimate partner violence, men and women's adherence to conservative norms, and their second order beliefs about their community's acceptance of intimate partner violence in various scenarios. Each respondent also participates in two incentivized decision-making activities to obtain a measure of allocative efficiency within a household, as well as a list randomization elicitation to cross validate survey measures of intimate partner violence. Finally, enumerators monitor whether individuals offered employment take up the job, their productivity at work, and how long they remain employed.

The structure of the study allows me to provide causal estimates of the impact of a job offer on women's experiences of intimate partner violence by comparing outcomes between women who were offered employment and women who were nominated for employment but not offered a job. I am also able to provide descriptive evidence on the characteristics that motivate take-up of these jobs, including whether a woman's past experiences of intimate partner violence play a role

in her willingness to accept work. Comparing outcomes between households where women were nominated and received job offers, to households where men were nominated and received offers, allows me to contrast intimate partner violence changes when women work against the status quo of male earned income. Finally, looking at patterns in outcomes of intimate partner violence, beliefs regarding violence, and intra household allocation allows me to provide suggestive evidence on the motivation behind violence in the household.

It is important to note that the sample of female employees in this study, who were all nominated by male business owners in their social networks, may not be representative of working women in Bangladesh. The preferences of mobile money business owners over the set of women they are willing to hire, and of women and their households regarding their labor force participation, interact to create a selected sample of potential female employees. These women likely have stronger labor force attachment than the average woman in Bangladesh, among other differences. I can compare the observable characteristics of female potential employees, including their experiences of intimate partner violence, to the characteristics of the wives of male potential employees—who may still not be representative of married women in Bangladesh—to identify any differences in observable characteristics that may be salient when considering the external validity of these results.

The rest of the paper is as follows: section [2.2](#) provides a detailed overview of the literature on intimate partner violence and female labor force participation. Section [2.3](#) describes the study and the larger experiment of which it is part, while section [2.4](#) presents the data on outcomes of interest collected during baseline fieldwork. Section [2.5](#) describes the econometric specification to be used for analysis, including the choice of control variables. Section [2.5.3](#) presents power calculations which show the minimum effect sizes I can detect given the sample size. Finally,

section 2.6 describes the next steps for the project and concludes.

2.2 Literature

2.2.1 Women's Labor Force Participation

The impacts of women's labor force participation can be difficult to quantify given that the decision to enter the labor force is endogenous to women's characteristics, their expected earnings, and household structure, among other factors. Still, women's labor force participation is associated with a variety of benefits. Broadly, the literature shows that women's labor force participation improves child health, increases girls' education, delays women's marriage and age at first birth, and improves women's ability to make decisions within their households [50]. These benefits may arise through several channels. First, women's labor force participation may simply relax the household budget constraint, allowing women to increase their consumption, and spend more on "luxury goods" such as daughters' health and human capital. Second, earning income can change a woman's bargaining power within the household. Contributing a larger share of a household's income can give women's preferences more weight in the household decision-making calculus, resulting in household decisions more aligned with their wishes. Finally, improvements in young girls' outcomes in households where older sisters or mothers work may be due either to the increased income in the household, or due to the higher expected returns to investments in girls caused by the future availability of suitable work.

In practice, several mechanisms are usually at play. Heath and Mobarak [51] show that proximity to garment manufacturing firms in Bangladesh—which are a major source of employment for women—increased girls' education, and delayed marriage. These changes stem from

both increased household income through the extensive margin of labor force entry of mothers and older sisters, as well as changing expected returns to investments in younger girls' human capital through the promise of future well paid work. Work by Qian [83] in China leverages exogenous changes in the relative price of crops cultivated by women compared to crops cultivated by men and finds that the increase in working women's relative wages improved survival of girls at birth, and increased the education of both girls and boys. In contrast, an increase in male wages decreased girls' survival rates and educational attainment. These results can be explained by women's earnings giving them more decisionmaking power within the household, or through improved earnings for women changing the expected returns to investments in daughters' human capital. Newman [77] finds that the creation of jobs for women through the Ecuadorian cut flower industry leads to changes in household time allocation, with men increasing time spent on unpaid household work. This may be a result of women's earnings giving them more bargaining power within the household.

The previous studies examined the impact of female labor force participation in households where women entered the labor force or where working women experienced exogenous increases in their earnings. However, women need not necessarily work to reap these benefits; several studies show that just the possibility of work can be sufficient to improve outcomes, both for women eligible to work, as well as girls who will be eligible for work in the future. The option value of a job can change a woman's threat point when bargaining with her partner, leading to different compromises within the household without requiring women to participate in the labor force. The promise of future work may also increase the expected returns to current investments in girls' health and human capital. A study in Mexico by Majlesi [71] considers the impact of exogenous labor demand shocks in different sectors of the economy on women's empowerment.

The author finds that exposure to positive shocks to labor demand in sectors that employ women increases the net number of decisions that women make within their households. Importantly, this increase in decision making power is not limited to women who enter the labor force: women who are exposed to shocks but who do not take up jobs also report increases in decisionmaking ability. These results are likely a consequence of both working women's increased control over household income, and changes in threat points for all women exposed to demand shocks.

Work by Oster and Steinberg [79] in India highlights how the promise of of suitable work for women in the future changes current investments in girls' human capital. The authors show that proximity to potential job opportunities for women in the business processing sector in India increases investments in girls' human capital. Similarly, an intervention by Jensen [61] which provides families information about job opportunities for young women increased the number of women who delayed marriage and increased investments in their human capital, compared to households who lived near the same job opportunities but were not provided information. The information intervention also led to increases in young women's aspirations for future work, and reduced ideal levels of fertility.

2.2.2 Women's Work and Intimate Partner Violence

Despite these benefits, studies have shown a positive correlation between women's labor force participation and intimate partner violence. Ex ante, the direction of the relationship between female labor force participation and intimate partner violence is uncertain; effects depend on the underlying motivations for intimate partner violence. Researchers have developed various frameworks with which to categorize the rationales for violence. Some categorize motivations

by the length of time from the violent event: those that arise from the perpetrator's background and past, contemporaneous contextual details, and immediate triggers. Others differentiate motivations for violence by frequency and severity of the violence itself.

Tauchen et al. [100] propose an economic model of intimate partner violence which broadly classifies motivations for violence as expressive or instrumental. Instrumental motivations posit that violence is used as a tool to gain or maintain control of household resources. Violence related to women's work may not solely concern monetary resources, but may be a way for men to control their partners' behavior. Coercion may decrease a woman's ability to make decisions, including decisions about her work. In contrast, expressive models of intimate partner violence posit that violence is used as a communicative signal to express emotions. Violence is a way for men to express anger, jealousy, unhappiness, or a reaction to stress.

Eswaran and Malhotra [37] formalize a non-cooperative bargaining model of intimate partner violence in a way which allows for heterogeneous changes to levels of household violence after women enter the labor force. The authors conceptualize violence as a tool used solely by husbands when spouses bargain over surplus in the marriage. Intimate partner violence allows husbands to increase their control of household resources by reducing female autonomy. As long as the level of violence employed by a woman's husband does not reduce her utility past a minimum threshold, a woman's labor force participation—a way in which she exercises her autonomy and earns income which can increase total household surplus—will increase the level of violence she faces. If use of violence either does not sufficiently increase a husband's share of household surplus, or if women have a high reservation utility, the use of violence should not increase in response to women's labor force participation.

The impact of a job offer to a woman on levels of intimate partner violence when violence

is instrumental in nature can either be positive or negative. If women's work in the formal sector generates additional income, violence motivated by competition over a small pool of household resources could decrease given that the household budget constraint relaxes. Women's earned income could also improve her ability to leave a marriage, and reduce the coercive power of intimate partner violence, as documented by Aizer [3] in California. Indeed, women who are most at risk of intimate partner violence may choose to enter the labor force as a way to gain financial independence and leave their relationships. Still, conflict over control of additional household resources could increase intimate partner violence.

Similarly, when violence is expressive in nature, women's labor force participation can either increase or decrease levels of violence. The additional income generated by women's work could reduce stress and unhappiness that arise from resource constraints, and therefore reduce violence. Alternatively, a woman's labor force participation may increase her husband's use of violence as a means to express anger at her for violating social norms by interacting with men in public, or taking on non-traditional gender roles of wage-earners. Violence does not only serve as a way to communicate with a spouse: a husband's use of violence may also signal to his peers that he has control of his household members. Ultimately, these rationales are not mutually exclusive: violence may be both expressive and instrumental, and may be related to both the level and allocation of household resources and women's behavior.

Empirical work has generally found a positive correlation between women's labor force participation and intimate partner violence across developing countries. Work by Heath [49] in Bangladesh shows that women who work in the garment sector are more likely to experience intimate partner violence. A paper by Amaral et al. [5] shows that participation in India's workfare program, NREGS, increases the incidence of intimate partner violence towards women. Davila

[29] studies Mexico's maquilas, which are a large employer of women, and finds that an increase in the share of female employment increases the number of divorce filings by women which cite intimate partner violence as a reason for divorce. The study also finds that an increase in the share of female employment increases female homicides. Erten and Keskin [36] use variation in the entry of Syrian refugees across the Turkish border as a negative labor demand shock for women in Turkey. The authors find that as female labor force participation declines among Turkish women, so do rates of intimate partner violence. Finally, Kotsadam et al. [68] leverage variation in female employment opportunities caused by mining activity across sub-Saharan Africa and find that intimate partner violence is largely unchanged by women's labor force participation. Even so, the authors find that there is significant heterogeneity in the effect of female labor force participation: if a locality has a high initial tolerance for intimate partner violence, increased female labor force participation increases intimate partner violence. While many of these studies leverage sources of variation exogenous to intimate partner violence, they may not be able to fully account for the impacts of selection into the labor force. If the women who are most likely to experience intimate partner violence are the same ones who select into the labor force, that could explain the positive correlation in the literature.

Randomized evaluations are able to provide causal estimates of the impact of job offers on intimate partner violence among women who have selected into the labor force. Existing randomized control trials offer mixed evidence on the impact of job offers to women on their experiences of intimate partner violence. Hjort [54] offers women in Ethiopia the opportunity to work in the cut flower industry, and finds a 19 percentage point increase in treated women's experiences of intimate partner violence. However, another experiment in Ethiopia by Kotsadam and Villanger [67] which randomly offers jobs to female applicants at different manufacturing

firms does not find that there are any increases in the incidence of physical intimate partner violence, and finds a decrease in the incidence of emotional abuse. Additionally, work by Green et al. [44] who randomly provide Ugandan women with entrepreneurship training, capital, and mentorship, finds that promoting business ownership among women does not increase intimate partner violence. Finally, work by Perova et al. [81] finds no change in intimate partner violence among women who were randomly selected to participate in Lao PDR's public works program.

The range of results from apparently similar experiments likely stem from differences in the type of work and the context in which these studies take place. Women who apply for unskilled work (cut flowers, road construction) likely have different outside options, budget constraints, and demographic characteristics than women who pursue more skilled work (in manufacturing, entrepreneurship). Working single-sex environments (road construction) may be more socially acceptable than working alongside men (manufacturing, cut flower industry). Men's employment and earnings may also matter: if women's work causes them to out-earn their partners, conflicts may be more severe than if women earn less than their spouses. Lastly, different levels of adherence to conservative social norms and baseline levels of intimate partner violence—which vary with socioeconomic characteristics—are important mediating factors.

This study takes place in a setting where men's labor force participation is relatively high, as did previous RCTs on female labor force participation and intimate partner violence. However, women's labor force participation is significantly lower than in previously studied contexts; women who accept job offers through this study may be making a larger change than in other studies, perhaps engendering more conflict within the household. Bangladesh is also more a conservative country compared to previous studies. Social norms emphasize female modesty and limit women's participation in the public sphere. Offering women employment in shops where

they will interact with men may represent a strong violation of social norms, leading to increased intimate partner violence. However, the nature of the work this project offers is different than the interventions in Ethiopia and Lao PDR, and is markedly different than the typical jobs available to women in Bangladesh. Working in a mobile money business can allow women to work flexible hours close to home, reducing conflict from the changing labor/leisure allocation in the household. The lower levels of earnings from part-time work may also make it unlikely that women will out-earn their husbands, a known source of conflict. While the nature of the work is most similar to the intervention by Green et al. [44] (working in a small enterprise), key differences are that women will not be business owners with decision making capacity or access to capital—and the accompanying risk—but will instead be employees.

2.2.3 Cash Transfers and Intimate Partner Violence

It is important to note that while female labor force participation has been shown to be positively correlated with intimate partner violence in developing countries, unearned income has a different effect. Randomized evaluations from Bangladesh [85], Kenya [47], Ecuador [53], and Mali [52] show that cash transfers are broadly associated with reductions in intimate partner violence. These results are robust to varying who in the household receives the transfer [47], and whether the transfer is given in cash or in kind [53]. One exception to these findings comes from Angelucci [13], who finds that when men strongly identify with traditional gender roles, cash transfers to women can increase women's experiences of intimate partner violence. The main reason why cash transfers may have a different effect on levels of intimate partner violence than earned income is that these transfers directly relax household budget constraints without reduc-

ing (and possibly allowing for an increase in) household leisure, or risking violation of social norms by women working or forgoing household chores to earn a wage. Finally, if these transfers are conditional or are accompanied by information that suggests conditionality (ie, nutrition education for children), there may not be conflict around usage of funds.

2.3 Experimental Design

This study is part of a larger experiment by Giné et al. [43] which aims to increase women's participation in the labor market and their use of mobile money. In Bangladesh, many mobile money transactions can only be completed with an agent, 95% of whom are male. Given the country's strong social norms against men and women interacting in public, this may be a significant barrier keeping women from using mobile money services. Increasing the fraction of women working as mobile money agents may increase the number of female mobile money customers. To do so, the study approached existing mobile money agents who were interested in expanding their operations by hiring an additional worker, and offered them a six month wage subsidies to offset hiring costs.

At the recruitment stage, enumerators conducted a census of over 5,700 shops in 870 bazaars to identify existing owners of mobile money businesses. After the census was complete, 618 bazaars had at least one eligible mobile money business that wanted to expand, and fit the criteria for inclusion. Enumerators asked one randomly selected eligible mobile money business owner from each bazaar to nominate two male and two female potential employees. These candidates had to be over the age of 18, could not live under the same roof as the business owner, and had to be available to join the shop if hired. Ultimately, 307 business owners successfully

referred four candidates that met the study's criteria.

The study entered each enrolled business owner in two independent lotteries. The first lottery determined whether the business owner received a six month wage subsidy which would offset the costs of hiring an additional worker. The second lottery selected which of the four nominated individuals the employer was obligated to hire. Subsidy payments were contingent on both the business owner hiring the worker identified in the second lottery, as well as the employee's compliance with monitoring visits by study enumerators to verify that new employee was working.

This paper uses supplementary data collected from the potential employees and their spouses to understand how the offer of employment changes the incidence of intimate partner violence. I leverage the second lottery from the main study to obtain causal estimates: the individual who is offered a job is randomly selected from the sample of individuals who were both considered by business owners for work in their businesses, and who were interested and available to work. Because which available individuals were actually offered jobs is determined randomly, I can attribute any change in the incidence of intimate partner violence to the job offer, rather than to characteristics that motivate an individual to enter the labor force. I plan to use a difference in differences specification in my analysis, which will regress outcomes related to the incidence of intimate partner violence on an indicator for the individual being randomly assigned to receive a job offer, an indicator for the gender of the potential employee, and the interaction between the two. This specification allows for job offers to male employees—the status quo—to have different impacts on intimate partner violence against women than job offers to female potential employees, while controlling for differences in the baseline incidence of violence between households where male and female candidates were nominated. Offering a female candidate a job is a sig-

nificant departure from the norm, as only 34.2% of female potential employees work at baseline. The changes in a household's income and labor/leisure allocation are likely very different than if a male candidate—87% of whom work at baseline—accepted a job in a mobile money business. Additionally, women's entry into the labor force is more likely to violate social norms. Together, these differences imply that the impact of a job offer to a female candidate will have different implications for the female household member's experience of intimate partner violence than a job offer to a male candidate. I discuss my hypotheses about the effects of treatment in section [2.5.1](#)

Surveying both potential employees and their spouses allows me to provide descriptive evidence of women selecting on observable characteristics, including the level of cooperation between spouses in the incentivized decisionmaking activities, the baseline level of intimate partner violence, and own ability, when deciding to enter into the labor force. I can compare the characteristics of female potential employees—who both were chosen by the business owner and agreed to be nominated for a mobile money job—to the characteristics of the spouses of male potential employees—who did not select into the labor force—to do so. While the spouses of male potential employees are not necessarily representative of the population of married women in Bangladesh, I can determine whether there are any salient differences between households where male and female potential employees were nominated, and assess the implications of such differences on whether the study's results would hold among a more representative sample of women in Bangladesh.

2.3.1 Fieldwork

Baseline survey activities to recruit existing mobile money business owners into the main study began in November 2021. Between February and mid-April 2022, enumerators from Innovations for Poverty Action, Bangladesh, began the process of verifying the eligibility of potential employees and conducting baseline surveys with them. Enumerators also conducted surveys with the spouse of each nominated potential employee ¹. The survey included questions on work experience, household characteristics, and familiarity with mobile money. Additionally, each respondent completed two incentivized decision-making activities, and answered a set of questions about intimate partner violence. Ultimately, 1,228 potential employees were enrolled and surveyed. Enumerators were able to interview the spouses of 765 potential employees.

2.4 Data

The baseline sample consists of 1,228 potential employees, of whom half are female. Approximately one third of the male potential employees are married (204 out of 614), while a little over one half of female potential employees are married (330 out of 614). The probability that the potential employee's spouse is also a household member varies by the gender of the potential employee: 93% (194) of married male potential employees' wives also live in the household, compared to 72% (238) of married female potential employees' husbands. This asymmetry is consistent with the high levels of male migration in Bangladesh. My analysis includes set of married candidates whose spouses are household members. In the rest of this section, I first present

¹If the individual is unmarried, enumerators survey a household member of the opposite gender who is most involved in decision-making.

the observable characteristics of female and male respondents, looking separately at potential employees and the spouses of potential employees. I then look separately at female respondents' experiences of intimate partner violence, and examine whether there is heterogeneity related to observable characteristics. I also look at respondents' adherence to conservative norms and second order beliefs regarding their community's sanction of intimate partner violence, looking separately by gender and potential employee status. Finally, I examine the decisions respondents made in each incentivized decisionmaking activity, again differentiating by gender and potential employee status.

2.4.1 Summary Statistics

I present summary statistics which describe the characteristics of female respondents (married female potential employees and the wives of married male potential employees) in table 2.1. On average, married female respondents are between 27 and 28 years old, and have been married to their husbands for 10 years. Married female potential employees are more likely to work outside the home than the wives of male potential employees: 34.2% of female potential employees work outside the home, while only 20.7% of the wives of male potential employees do so. Similarly, rates of job search are much higher among female potential employees, 66.7% of whom are trying to find a new job. In contrast, only 14.3% of the wives of male candidates are seeking work. No woman reports having had five or more jobs in her lifetime.

Rates of phone ownership range from 65.2% of the spouses of male potential employees, to 90% of female potential employees. Conditional on phone ownership, SIM card ownership ranges from 30.1% of the wives of male potential employees to 47.8% of female employees.

This appears consistent with the fact that only 77.6% of female potential employees and 71.4% of the wives of male employees have a national identity card (NID), which is legally required in order to purchase and activate a SIM card.

Use of contraception is high in this sample; 79.4% of the wives of male employees and 85.1% of female potential employees have ever used contraception. Given the strong historical government promotion of family planning programs, this fact does not seem at odds with 67.9% of female candidates and 73.9% of the wives of male candidates describing themselves as observing a religious conservative lifestyle.

Finally, I find that few respondents' mothers or mothers-in-law have worked for pay in or out of the home. Among female candidates, I observe that 14.3% of their mothers ,and 13.1% of their mothers-in-law ever worked for pay outside the home. Only 9.9% of the mothers of wives of male candidates, and 9% of their mothers-in-law ever worked for pay outside the home. Comparatively, the levels of labor force participation for married women in this sample is much higher.

In table 2.2, I present the characteristics of the husbands of female potential employees and male potential employees. On average, the husbands of female potential employees are 37.5 years old, and male candidates are 35.39 years old. Most male respondents work; 82.7% of the husbands of female potential employees and 89% of male potential employees work outside the home. Rates of on the job search appear high as well, given that 31.7% of the husbands of female potential employees and 80.2% of male potential employees are searching for work. Nearly all male respondents have had fewer than 5 jobs in their lifetime.

NID and phone ownership among male respondents is practically universal, and about half of male respondents own smartphones. Conditional on owning a phone, male respondents are

more likely to have a SIM card registered in their own name than are female respondents; 82.7% of the husbands of female potential employees, and 88.8% of male potential employees have a SIM registered in their own name.

Finally, fewer male respondents than female respondents report that they are religiously conservative; in fact, 36% of men married to female potential employees state they live a modern lifestyle, as do 24% of male potential employees. Still, the levels of their mothers' and mothers-in-law's labor force participation are very low, topping out at 9.4%.

2.4.2 Intimate Partner Violence

Only female respondents were asked questions about their experiences of intimate partner violence. Rates of intimate partner violence in this sample are comparable to rates found in other studies of married women in Bangladesh. I present statistics from survey questions in table 2.3. On average, 78% of currently married women in this sample have ever experienced any form of intimate partner violence, ranging from coercive behaviors to verbal and physical abuse, to sexual assault. Currently married female potential employees report slightly lower rates of intimate partner violence (77%) than the spouses of currently married male employees (85.4%). When I consider habitual coercive behavior and instances of intimate partner violence which occurred within the last six months, rates are marginally for both female candidates and wives of male candidates. 73.2% of female candidates report experiencing intimate partner violence in the past six months, while 82.2% of married candidates report the same. The small difference between the probability a respondent has ever experienced intimate partner violence and the probability that a respondent has experienced intimate partner violence in the past six months shows that IPV

is a persistent problem for women in this sample.

Physical violence is less common than verbal abuse; 32.5% of currently married potential female employees report ever having experienced physical violence at the hands of their partners, while 27.6% of wives of male potential employees have experienced the same. When I consider violent acts within the past six months, I find that 19.4% of married female candidates have been subject to physical violence, and 15.5% of the wives of male candidates have experienced physical violence committed by their husbands.

While not causal, table 2.4 shows currently married female potential employees who work are twice as likely to have experienced physical violence at the hands of their partners than currently married female potential employees who are not in the labor force. The 34.2% of currently married potential female employees who work outside the home report ever experiencing physical violence at a rate of 42.9%, compared to 26.8% by currently married female potential employees who are not currently working. The same does not appear to be true for the wives of currently married male potential employees: though the rate of labor force participation is much lower (20.7%), the reported rates of physical violence are statistically indistinguishable, with 27.3% of currently working wives of male candidates experiencing physical intimate partner violence, compared to 28% of wives of male candidates who do not work outside the home. When I consider physical violence in the past six months, differences in the incidence of violence by candidate status and labor force participation are slightly smaller. 25.9% of married female potential employees who work for pay have experienced physical violence in the past six months, as have 16.2% of married female potential employees who are not working. Rates of physical abuse in the past six months are nearly indistinguishable between the wives of male candidates who work (15.8%) and the wives of male candidates who do not work (15.6%).

2.4.3 List Randomization Elicitation

Conflict within a marriage is a sensitive subject which respondents may be reluctant to discuss with enumerators. Women may be unwilling to tell a stranger about difficult moments within their marriage, due to lack of trust or embarrassment. Therefore, survey-based estimates of intimate partner violence could underestimate its true prevalence. Indirect elicitation methods, such as list randomization, can improve measurement of sensitive outcomes when respondents may not be forthcoming with enumerators. Using a list randomization elicitation to measure intimate partner violence was first used by Joseph et al. [62] in a study in Kerala, and has been used by many researchers since [2, 67, 82]. The format of the elicitation allows an individual respondent to share their experiences of intimate partner violence *without the enumerator knowing that she has experienced intimate partner violence*. In theory, this should increase the likelihood that a woman shares her experiences truthfully [67]. With this method, I can only measure the prevalence of intimate partner violence in the sample, and do not know whether a particular respondent has experienced intimate partner violence.

The elicitation is implemented as follows: first, the enumerator asks each respondent to put her hand behind her back and make a fist. Next, the enumerator reads out a list of statements, and the respondent is asked to extend a finger for each statement that she feels applies to her. After all statements on the list have been read out, the enumerator asks the respondent to bring her hand to the front and show how many fingers she has extended.

All respondents are read four identical statements; crucially, half of the respondents are randomly selected to hear a fifth statement, which states that they have ever experienced physical violence inflicted by their partners. The average number of the first four statements that apply

to women should be the same for both groups. Hence, the difference in the average number of statements affirmed by the group that hears five statements and the group that hears four statements should indicate the proportion of women who believe the statement about intimate partner violence applies to them. The full text of the protocol is available in appendix [B.3](#).

The correspondence between survey measures of intimate partner violence and the list randomization elicitation is high; results are in table [2.5](#). Among married female potential employees whose husbands live in the household, 27.4% report that their husbands have ever kicked, slapped, or punched them, when responding to the survey. The corresponding rate implied by the list randomization measure is 23.6%. Similarly, for the wives of married male potential employees, 19.4% directly respond to survey questions and indicate that their husbands have inflicted physical violence, while the list randomization experiment implies a rate of 17.7%. I cannot reject that the rate of IPV measured by survey questions is statistically significantly different than the rate implied by the list randomization elicitation in either sample. Given the degree of correspondence between survey based measures and other surveys in Bangladesh, as well as the correspondence between the list randomization elicitation and survey measures, I use survey measures as outcomes of intimate partner violence in the remaining analysis.

2.4.4 Perceptions of Intimate Partner Violence

In addition to asking women their experiences of intimate partner violence, I ask both male and female respondents whether they agree or disagree with conservative social norms regarding women's behavior, as well as their second order beliefs regarding their community's sanction of intimate partner violence in several scenarios. I present summary statistics from this set of

questions, differentiating by respondent gender and potential employee status, in table 2.6. The first two columns present results from female candidates and their husbands, and the last two columns present results from the wives of male candidates and male candidates.

Over 95% of respondents—regardless of gender or candidate status—agree that wives should be obedient to their husbands, and over 90% agree that family problems should not be discussed outside of the household. 88.7% of male candidates and 85% of husbands of female candidates agree that men should show their wives who is in charge, but female potential employees are significantly less likely to agree with this statement (58.9%), while the wives of male potential employees fall in between, at 78.4% agreement. 70.3% of male potential employees agree that if women earn more than men, it is likely to cause problems, while 52.2% of the husbands of female potential employees agree with the same statement. Female potential employees and the spouses of male potential employees agree with the statement 44.9 and 48.5% of the time, respectively. Interestingly, women are nearly twice as likely to believe that a wife is obligated to have sex with her husband even if she doesn't feel like it (46% for both female potential employees and wives of male potential employees) than men (26% for both male potential employees and husbands of female potential employees); there are no significant differences by candidate status. Finally, only about a third of all respondents believe that individuals outside the family should intervene if a husband mistreats his wife, with no meaningful differences by gender or potential employee status.

Respondents' second order beliefs about whether their community justifies intimate partner violence varies across scenarios. However, there are very few systematic differences depending on gender or status as a potential employee. 18.3% of female potential employees and 19.6% of the wives of male potential employees believe that their community justifies intimate partner

violence if a woman goes out without telling her husband. Among male respondents, 23.4% of male candidates believe that their community believes violence is justified in this case, whereas 16.4% of the husbands of female potential employees agree with the same statement.

A plurality of respondents believe that their community supports violence if a wife neglects the house or children. Among female respondents, 25.9% of potential employees and 35.1% of the wives of male potential employees believe their community supports this view. Among male respondents, 25.4% of candidates believe their community justifies violence in this instance, and 22.3% of the husbands of female potential employees agree with the statement.

When asked whether their community justifies intimate partner violence if a wife argues with her husband, around one quarter of respondents agree. Among female respondents, 27.6% of candidates agree that their community holds this view, while 30.9% of the wives of male candidates believe so. Among male candidates, 24.1% of potential employees agree, while 19.8% of the husbands of female potential employees believe their community justifies intimate partner violence for this reason.

There is less agreement that the community justifies intimate partner violence if a wife refuses to have sex with her husband. Among female respondents, 7.1% believe their community justifies intimate partner violence in this case, while 12.4% of the wives of male potential employees believe so. Among male respondents: potential employees agree their community holds this view about 9.2% of the time, while the husbands of female potential employees only believe so 3.6% of the time.

When asked whether their community justifies intimate partner violence if a wife does not cook food properly, rates of agreement are low. Female potential employees and spouses of male potential employees believe this is the case 6.1 and 7.2% of the time respectively, while male

potential employees and husbands of female potential employees agree their community holds this view 7 and 10% of the time respectively.

The penultimate situation respondents consider is whether their community justifies intimate partner violence in the event a husband suspects his wife of infidelity. 11.2% of female employees agree with the statement, while 5.2% of the wives of male potential employees do so. Among male respondents, 12.8% of potential employees believe their community holds this view, while 14.5% of the husbands of female potential employees believe so.

Finally, respondents are asked whether their community justifies intimate partner violence if a wife disrespects her in-laws. Female potential employees and the wives of male potential employees agree with this statement 31.1 and 38.1% of the time respectively, while male potential employees and husbands of female potential employees believe so 31.5 and 30.3% of the time.

Broadly, the wives of male potential employees appear to hold more conservative views than female potential employees; the equivalent holds among male respondents, where male potential employees hold slightly more conservative views than the husbands of female employees. However, differences within gender by candidate status are not statistically significant.

2.4.5 Incentivized Decisionmaking Activities

Incentivized decision making activities (IDM) are survey-based activities which ask respondents to make allocation decisions over a real pot of money [4, 6, 8]. Decisions made in these activities correspond to those made outside of the survey, and can shed light on the types of allocation frictions within households [4]. In the baseline survey, respondents complete two different incentivized decision making activities. Prior to beginning the module, enumerators

inform respondents that one of the four IDM activities completed by the household—the two the respondent completed, and the two that their spouse completed—will be selected for payment at the end of the survey. After both the respondent and their spouse completed the baseline, the enumerators jointly give the respondent and their spouse payment in a sealed envelope. This is to ensure that neither respondent knows which activity was paid out, and to decrease the chance of conflict in the household due to IDM allocations.

In this section, I focus on results for the subsample of 502 respondents from 251 households who are currently married, whose spouses live in the same household and were successfully surveyed.

For the first incentivized decisionmaking activity, the enumerator begins by asking the respondent whether they would prefer 110 taka be given to the respondent directly or 110 taka be given to their spouse. Next, the enumerator repeats the question, always presenting a choice between 110 taka given to the recipient the respondent did not initially select, or a smaller amount given to the respondent's preferred recipient, lowering the amount allocated to the respondent's preferred recipient by 10 taka at a time. The questions continue until either the respondent selects a different recipient or chooses an allocation with 0 taka given to one partner. The full text of the protocol is in appendix [B.1](#).

I present a brief example of how the protocol may play out. Suppose the respondent elects to receive the transfer themselves when initially asked in the first activity. The next question then asks whether the respondent prefers to receive 100 taka themselves, or for 110 taka to be given to their spouse. Here, if the respondent chooses for 110 taka to be given to their spouse, the activity ends. If the respondent chooses again to receive the transfer themselves, the question is repeated, this time asking whether the respondent prefers 90 taka given to themselves, or 110 taka given to

their spouse. The activity continues in this way until either the respondent switches recipients, or the amount to the respondent's preferred recipient goes to zero (10 iterations).

This first activity is designed to measure how much the respondent is willing to give up to maintain their preferred allocation of the endowment. If the household operates efficiently, the respondent should not give up any amount of money in order to have the pot given to their preferred recipient; the respondent and their spouse can make transfers after the experiment to divide the endowment in line with their preferences. However in the presence of household frictions, giving up a portion of the payment to maintain a particular allocation may be optimal.

The second incentivized decisionmaking activity asks the respondent to privately decide how much of a 110 taka endowment should be given to the respondent, and how much should be given to the respondent's spouse. This game measures the level of generosity between the respondent and their spouse. The full text of the script is in appendix [B.2](#).

Table [2.7](#) presents statistics from both IDM activities. The first two columns present results from female candidates and their husbands respectively, while the last two columns present results from the wives of male candidates and male candidates respectively. Half (49.6%) of currently married female potential employees chose to receive the initial allocation themselves. The husbands of married female candidates choose to receive the initial allocation themselves rather than give it to their spouses 52.5% of the time. The wives of currently married male potential employees chose to receive the initial allocation themselves only 41.1% of the time, while 63.1% of married male candidates choose to receive the initial allocation themselves.

On average, female potential employees gave up 29.5 taka in order to keep their initial allocation preferences. In other words, the average female candidate preferred that the enumerator hand over 80.5 taka to the female candidate's preferred respondent, rather than have 110 given to

the person the respondent did not initially select to receive the transfer, and then redistributing the money outside of the game. The husbands of female candidates gave up 27.63 taka to ensure that the money was given directly to their preferred recipient. Wives of male candidates gave up 26.88 taka on average to keep their initial allocation preferences, while married male candidates gave up 33.6 taka to maintain their initial allocation decision. These statistics suggest that respondents experience frictions when making decisions about allocating money with their spouses. Indeed, respondents are willing to give up between 24.4% and 30.5% of an endowment worth approximately half a day's wages for a garment factory worker to ensure the enumerator gives money to the person of their choice. Both male and female respondents face frictions, regardless of whether they are a potential candidate. The activity does not tell us whether these frictions stem from spousal conflict over money, self-control issues, or strong adherence to norms around money. Looking at how the impact of a job offer on women's experiences of intimate partner violence varies based on the level of baseline allocation frictions can provide insight regarding the source of allocation frictions, as well as the motivations underpinning intimate partner violence in the household.

A significant fraction of respondents maximized the payment to their household in the first incentivized decision making activity and did not give up any amount of money to maintain their initial allocation decision. Among married female potential employees, 45.3% maximized the transfer to their household in the first activity, while their husbands maximized transfers to the household 49.6% of the time. In households where a male candidate was nominated, the wife of the male candidate maximizes the transfer 49.1% of the time, while the male candidate maximized the transfer to the household 42% of the time.

However, a substantial number of respondents never switched the recipient of the trans-

fer, giving up the maximum amount to keep their preferred recipient. Among female potential employees, this fraction is 25.9%. Their husbands—the spouses of potential female employees—refused to change their allocation 24.5% of the time. Wives of male potential employees are never willing to change the transfer recipient 24.1% of the time, and male potential employees refuse to switch the transfer recipient 30.4% of the time. Respondents who refused to change their allocation decision may have very strong preferences, either due to frictions within the household that prevent them from maximizing transfers, or because they want to communicate something about themselves to the enumerator in this activity (ie, that they are generous to their spouse to their own detriment). Alternatively, these respondents simply may not have understood the activity. Finally, it is also possible that these respondents did not treat this activity seriously.

Allocations in the second incentivized decisionmaking activity are close to even on average. Female candidates keep 51.58 taka of the 110 taka pot for themselves, while their husbands keep 46.19 taka for themselves. Wives of male candidates keep 41.88 taka for themselves on average, while male candidates keep 57.12 taka of the 110 taka pot for themselves on average. Both respondents in households where a female candidate was nominated tend to be more generous to their spouses than in households where a male candidate was nominated. Female candidates give their spouse 58.42 taka on average, and their husbands give them 64.28 taka on average. The wives of male candidates give their husbands 68.13 taka in the second incentivized decisionmaking activity, while their husbands give them less—52.36 taka—on average.

Finally, I consider the fraction of respondents who either gave the entire pot of money in the second incentivized decisionmaking activity to their spouse, or who kept the entire pot to themselves. 12.9% of female potential employees and 20.3% of their husbands gave the entire pot to their spouse in the second activity, keeping nothing for themselves. When I consider

households where male potential employees were nominated, I find that 20.5% of the wives of male candidates give their husbands the entire pot, but men return the favor at half the rate (10%). I find that female candidates keep the entire pot for themselves 11.5% of the time, while their husbands keep the entire pot for themselves at half that rate (5.8%). In households where the potential employee is male, I find the opposite hold true: the wives of male candidates keep the entire pot only 5.4% of the time, while their husbands keep the entire pot 13.6% of the time.

Combined with the results on respondents' adherence to conservative social norms, and their second order beliefs regarding their community's justification of intimate partner violence, the data broadly suggest that the households of married male potential employees are more conservative and favor men more than the households of married female potential employees.

In table 2.8, I examine differences in observable characteristics based on respondents' behavior in the first incentivized decisionmaking activity, namely, whether they maximized the transfer to their household, or whether they gave up the entire pot to maintain their preferred allocation. The first two columns present characteristics of female potential employees who maximized the transfer to their households and those who gave up the entire pot respectively. I then consider differences between the husbands of female candidates who maximized transfers to their households, and those that gave up the entire pot in the first IDM activity. The last four columns consider respondents from households where a male potential employee was nominated. The fifth and sixth columns look at characteristics of the wives of male candidates who maximized transfers and gave up the entire pot respectively, and the last two columns look at the characteristics of male candidates who maximize transfers to their households, and those who gave up the entire pot to maintain their preferred recipient. All respondents except for male candidates are more likely to have initially chosen to receive the transfer in the first IDM if they maximized

the transfer to their household, compared to respondents who never switched recipients. 68% of male candidates requested to receive the initial transfer regardless of whether they immediately or never switched recipients. Otherwise, I find no systematic meaningful differences in observable characteristics based on a respondent's behavior in the first IDM.

In table 2.9, I show that respondents make consistent decisions between the two incentivized decisionmaking activities. Specifically, if respondents choose to receive the initial transfer themselves in the first IDM activity, they keep more of the pot for themselves in the second activity. This pattern holds true across gender and potential employee status. This logical consistency suggests that the activities measure dimensions of individuals' preferences over divisions of money between themselves and their spouses, and reflect allocation frictions they might face outside of the activity. In the first four columns of table 2.9, I consider decisions by respondents within households where female candidates were nominated, and in the last four columns I consider respondent decisions in households where male candidates were nominated. The first two columns in table 2.9 look at the decisions made by female candidates. As previously shown in table 2.7, half (49.6%) of female candidates choose to keep the initial transfer in the first incentivized decisionmaking activity to themselves. I then look at the decisions they make in the second activity differentially by their decisions in the initial part of the first activity. I find that female candidates who chose to keep the pot to themselves in the first incentivized decisionmaking activity question are much more likely to keep the entire pot to themselves in the second activity (20.3%) compared to those who wanted their spouse to receive the initial transfer in the first activity (2.9%). Similarly, I find that female candidates who chose to keep the initial transfer in the first incentivized decisionmaking activity give the entire pot to their spouse at a lower rate (7.2%) in the second activity than if they had chosen their spouse to receive the initial transfer in the first

activity (18.6%). Finally, I find that female candidates keep more of the pot for themselves in the second activity if they chose to receive the initial transfer in the first activity for themselves (61.01 taka) compared to the amount they keep for themselves if they asked for their spouse to receive the pot in the first activity (42.29 taka).

Results for the husbands of female candidates are similarly consistent, and are shown in the third and fourth columns of table 2.9. The 52.5% of husbands of female candidates who choose to receive the initial transfer in the first incentivized decisionmaking activity are more likely to keep the entire pot for themselves in the second activity (9.6%) compared to the 47.5% who chose their wives to receive the initial transfer in the first activity (1.5%). Similarly, husbands who chose to receive the initial transfer in the first activity are far less likely to give their spouse the entire pot in the second activity (6.8%) compared to husbands of female candidates who asked for their wives to receive the initial allocation in the first activity (35.4%). Finally, husbands who chose to receive the initial transfer in the first activity keep more of the pot for themselves in the second activity (60.69 taka), compared to the amount kept by husbands who asked for their wives to receive the initial transfer in the first activity (30.15 taka).

I next examine the decisions of respondents who live in households where male potential employees were nominated. I look at results for the wives of married male candidates in the fifth and sixth columns of table 2.9. Wives of married male candidates are slightly less likely to ask to receive the initial transfer in the first activity for themselves than all other respondents (41.4%). However, if they do, they are more likely to keep the entire pot in the second activity (8.7%) than if they had asked for their spouse to receive the initial transfer in the first activity (3.1%). Conversely, wives of male candidates who choose to receive the initial amount in the first activity are far less likely to give the entire pot to their husbands in the second activity (10.9% compared

to 27.7%). Lastly, wives who elect to receive the initial transfer in the first activity keep 53.48 taka for themselves in the second incentivized decisionmaking activity, compared to the 33.54 taka kept by wives who ask for their husbands to receive the initial transfer in the first activity.

Finally, I look at decisions made by married male candidates in the last two columns of table 2.9. Married male candidates elect to receive the initial transfer in the first incentivized decisionmaking activity 64.6% of the time, the highest rate over all respondents. For male candidates who ask to keep the initial amount in the first activity, the probability they keep the entire pot to themselves in the second activity is higher (16%) than if they had asked for the initial amount in the first activity to be given to their wives (11.1%). The reverse holds true as well: male candidates who elect to receive the initial transfer in the first activity are far less likely to give their wives the entire pot in the second activity (3.7%) compared to male candidates who nominate their wives to receive the initial transfer in the first activity (20%). Finally, male candidates who choose to receive the initial transfer in the first activity keep 67.07 taka of the 110 taka pot in the second activity, compared to the 43.11 taka kept by male candidates who ask for their wives to receive the initial transfer in the first activity.

In table 2.10, I show some differences between households where female candidates are nominated, and those where male candidates are nominated. These differences are not necessarily statistically significant, but they provide important descriptive evidence of factors that influence selection into the labor force by business owners and women. I find that it is less likely (16.4%) that both respondents choose to have the husband receive the initial transfer in the first incentivized decisionmaking activity in households where female candidates are nominated compared to households where male candidates are nominated (28.9%). I also find that the total amount given to the husband in the second incentivized decisionmaking activity (sum of amount husband

keeps for himself and amount wife gives to husband) is smaller in households where a female candidate was nominated (104.6 taka) compared to the amount given to husbands in households where male employees are nominated (125.3). I also find that husbands of female candidates adhere to fewer conservative norms (3.551) than married male candidates (4.492), and that husbands of female candidates are less likely to believe that their community ever justifies intimate partner violence (34.2%) compared to married male candidates (41.4%). Combining responses to survey questions about agreement with conservative social norms and beliefs regarding the community's sanction of intimate partner violence into an index shows the same pattern of results.

Even though rates of physical intimate partner violence appear higher in households where female candidates are nominated, these households appear to have more liberal husbands than households where male candidates are nominated. One explanation may be that households where women are candidates have already experienced—and resolved—conflict around women's work, earnings, and participation in society. Households where men are candidates may not have gone through similar conflict. However, given that a significant fraction of female candidates who work report having experienced intimate partner violence in the past six months, these conflicts may be ongoing, even in more liberal households.

2.5 Identification

The correlations in the baseline data suggest that there may be a positive relationship between female labor force participation and intimate partner violence for female potential employees who have previously been employed. However, the effect of an offer to work at a mobile money business on rates of intimate partner violence may be positive or negative, and may vary

based on the attributes of potential employees and their spouses. While previous work, including studies in Bangladesh, find that rates of intimate partner violence are higher when women participate in the labor force, there are reasons to believe that this study may not find the same results. The type of job offered to potential candidates is very different than the previous types of employment studied: schedules are more flexible, hours are shorter, the work is service-oriented (as opposed to skilled or unskilled labor) and the size of the establishment is significantly smaller. These jobs also require higher rates of customer interaction. The former characteristics may make it less likely that conflict increases in the household after women are offered jobs, as they may be more able to balance household responsibilities with paid work, but the latter may make it more likely that conflict increases, since women may be more likely to violate social norms against women interacting with unrelated men, especially in public.

In order to obtain causal identification of the impact of a job offer on the incidence of intimate partner violence against female household members, I leverage the random allocation of a job to one of the four potential employees nominated by each mobile money business owner. I plan to use the following specification for analysis:

$$Y_{ia} = \beta_1 + \beta_2 \times \text{Offered Job}_{ia} + \beta_3 \times \text{Female Candidate}_{ia} + \beta_4 \times \text{Offered Job} \times \text{Female Candidate}_{ia} + \mathbb{X}\beta + \epsilon_a$$

where i indexes the potential employee's household and a indexes the business owner who nominated the employee; all regressions will be clustered at the business owner level. Outcomes Y_{ia} measure intimate partner violence against the female household member, the male household

member's beliefs regarding norms around intimate partner violence and adherence to conservative norms, and the male household member's decisions in the incentivized decisionmaking process. While treatment is randomly assigned, I include the following control variables in $\mathbb{X}$ to improve the precision of my estimates: potential employee age, gender and education; spouse age, gender and education; housing characteristics at baseline; geography (district, division); number of children. For specifications related to incentivized decisionmaking activities, social norms, and beliefs regarding community sanction of intimate partner violence, I also plan to include baseline levels of outcome variables on the right hand side in order to measure the change in the outcomes caused by job offers.

The effect of a male potential employee receiving a job offer on changes in the female respondent's experience of intimate partner violence is given by β_2 . Any underlying differences in rates of intimate partner violence against the female household member between households where a female potential employee was nominated and those where a male potential employee was nominated should be captured by β_3 . Finally β_4 should measure the treatment effect when a female potential employee receives a job offer. The sum of β_2 and β_4 should measure the effect of a job offer to a female potential employee on her experiences of intimate partner violence.

Note that this is an intent-to-treat effect of a job offer: individuals who were offered a job and accepted are treated the same as individuals who were offered a job and declined to work. Analyzing take-up of job offers will be important to understand how past experiences of intimate partner violence and household cooperation affect a woman's willingness to work if offered a job. I plan to use the same difference in differences specification to understand how job offers correspond to labor force participation for men and women. In this case, the outcome variable will be an indicator for whether the employee began work within two weeks of receiving a job

offer. β_2 will measure the rate at which male candidates accept job offers, and the sum of β_2 and β_4 will capture the rate at which female candidates accept job offers. To understand what factors determine whether men and women accept job offers, I will regress the indicator for whether an employee began work on their gender, labor market history, women's experiences of intimate partner violence, and the male household member's adherence to conservative norms. Comparing coefficients will provide suggestive evidence about which factors are most important. This analysis will limit the sample to the set of employees who are offered jobs, which is a much smaller sample than the set of potential employees; as such, this analysis is likely to be underpowered to detect effects.

2.5.1 Hypothesized Effects

The data I collect allows me to provide suggestive evidence of the mechanism through which a randomized job offer affects women's experiences of intimate partner violence. I consider four main channels through which treatment may affect women's experiences of intimate partner violence: (1) violence is an expressive response to the stress of poverty, and is alleviated by women's earned income; (2) violence is an expressive response to women violating modesty norms through work, and increases after treatment; (3) violence is an instrument used to capture more household surplus and increases after treatment; and (4) violence is an instrument used to capture more household surplus and becomes less effective after treatment as women's outside options improve. I discuss each in more detail below. These mechanisms are not mutually exclusive; the reduced form nature of my analysis implies that a coefficient of zero could either imply that the treatment has no effect, or that multiple mechanisms with opposite effects are at

play. As such, examining different patterns of results on intimate partner violence, adherence to conservative norms, and men's allocations in the incentivized decisionmaking activities can provide suggestive evidence regarding which mechanisms apply, as can examining heterogeneous treatment effects which I discuss in further detail in section [2.5.4](#).

Expressive Violence, Poverty: If the baseline violence in the household is due to the stress of poverty, then a job offer to a female candidate should reduce the incidence of intimate partner violence. If I see that job offers to women decrease intimate partner violence, and that this effect declines as baseline household wealth increases, this would provide additional evidence to suggest that the mechanism through which the treatment operates is by reducing violence associated with the stress of poverty. I discuss how I plan to estimate heterogeneous treatment effects, including by baseline wealth, in section [2.5.4](#).

Expressive Violence, Violation of Modesty Norms: If violence is a response to women's perceived or real violation of social norms, I may see that female candidates are less likely to accept job offers, and that the acceptance rate may be lower among households with strong adherence to conservative social norms at baseline. I also might see that intimate partner violence increases after women are offered jobs, and that this increase is stronger among households where the male respondent strongly adheres to conservative social norms at baseline. Finally, I would expect higher quit rates among women who take up jobs whose husbands express agreement with conservative social norms.

Instrumental, Conflict over Surplus: If violence is used as a tool to control more surplus, I would expect job offers to women to increase women's experiences of intimate partner violence. I would also expect men whose wives receive job offers to be less generous to their spouses after treatment, being more likely to elect to receive the initial transfers in IDM1, and being willing to

give up more to retain control of the pot in IDM1. I would also expect men whose wives receive job offers to keep more of the pot for themselves in IDM2. I do not expect that treatment would change beliefs regarding norms around intimate partner violence if violence is instrumental in nature.

Instrumental, Women's Outside Option Improves: A job offer to a female candidate could improve her options outside marriage, and would render violence less effective as an instrument to increase control over surplus. In this case, a job offer to female candidates would decrease the incidence of intimate partner violence. However, I would expect changes in husbands' actions in the incentivized decisionmaking activities: as they have less scope to control surplus within the household, they may be more inclined to control money within the IDM activity. I would expect men whose wives receive job offers to be less generous to their spouses after treatment, being more likely to elect to receive the initial IDM1 transfer, and being willing to give up more to retain control of the pot in IDM1. I would also expect men whose wives receive job offers to keep more of the pot for themselves in IDM2.

I summarize the hypothesized pattern of coefficients for each mechanism in section [2.7](#). Below, I list each specific outcome variable I plan to include in my analysis:

Indicator variable =1 if:

- the candidate takes up the job offer
- the candidate stops work prior to 6 months due to family issues
- the female household member has experienced IPV, past 6 months
- the female household member has experienced physical IPV, past 6 months
- the male household member is transfer-maximizing in IDM1
- the male household member selects himself to receive the initial allocation in IDM1
- the male household member is willing to give up all of the transfer to maintain his initial allocation in IDM1
- the male household member keeps the entire pot in IDM2
- the male household member gives his wife the entire pot in IDM2

- the male household member believes the community ever justifies IPV

Additional Outcomes:

- the number of months the candidate works
- the amount of money the male household member is willing to give up to maintain his initial allocation in IDM1
- the amount the male household member keeps for himself in IDM1
- the number of social norms to which the male household member adheres
- an index measure of adherence by the male household member to conservative norms
- an index measure of the male household member's beliefs about whether the community justifies IPV

2.5.2 Multiple Hypothesis Testing

When testing multiple, related outcomes, there is a possibility that one set of results may be significant even though the true effect is zero, due to the 5% chance of Type I error. As such, it is important to correct p-values for multiple hypothesis testing to reflect that the outcomes are not independent and reduce the chance of spurious statistically significant results. I plan to adjust p-values using a procedure described by Romano and Wolf [84] and implemented in Stata [26]. This step-down procedure controls the family wise error rate (FWER, the probability of rejecting one true null hypothesis within a set of outcomes), allowing for strong control of the FWER without significant losses in statistical power. By using a bootstrap resampling procedure, the Romano-Wolf correction implicitly accounts for dependence among outcomes.

I plan to apply the Romano and Wolf procedure to outcomes within two distinct families of outcomes: women's experiences of intimate partner violence (intimate partner violence within six months of endline, physical intimate partner violence within six months of baseline), and male respondents' decisions within the incentivized decisionmaking activities (husband chose to receive initial transfer in IDM1 for himself, husband maximized transfer to household in IDM1, amount husband gave up to maintain preferred recipient in IDM1, amount husband kept for self

in IDM2).

2.5.3 Power Calculations

I perform power calculations using baseline data in order to measure the minimum effect sizes for each outcome variable that I can expect to detect after the intervention concludes. Results of these power calculations are presented in table 2.11. For my main measures of intimate partner violence—whether the female respondent has experienced intimate partner violence in the six months before the survey, and whether she has experienced physical violence in the six months before the survey—the smallest effects I can detect are a 31.5 percentage point change in whether the female household member has experienced intimate partner violence in the last six months, and a 29.5 percentage point change in whether she has experienced physical violence in the past six months, when I compare female potential employees who received offers to female potential employees who did not receive offers. These minimum effect sizes are larger than those found in a study by Hjort [54] in Ethiopia, one of the few RCTs that has found statistically significant increases in the incidence of intimate partner violence after women enter the labor force. When I use other comparison groups, the smallest effect size I can detect changes: comparing treated female potential employees with households where male potential employees receive job offers, the smallest effect I can detect is higher (given the smaller number of treated male employees), and comparing households where either a male potential employee receives an offer or no potential employee receives a job offer, the smallest effect I can detect is slightly lower (given the increase in the comparison group size).

I also look at the smallest effects I can detect among outcomes related to men’s decisions in

the incentivized decisionmaking activities. The smallest change in the amount the male respondent gives up to maintain his initial allocation in the first incentivized decisionmaking activity varies based on the comparison group; I can detect changes as low as 15 taka. Since the respondent's decision is made in 10 taka increments, this represents an additional 2 rounds of the first activity. The smallest change I can detect in the amount the male respondent chooses to keep for himself in the second incentivized decisionmaking activity is between 13 and 22 taka depending on the comparison group. I also consider binary variables: whether the respondent chose not to give up any money to maintain his initial allocation, whether the respondent gave up all of the money to maintain his allocation in the first activity, whether the respondent elected to receive the initial allocation in the first activity, and whether the respondent chooses to give the entire pot to his spouse (or keep the entire pot for themselves). The smallest effect sizes I can detect for these binary outcomes vary between 17 and 32 percentage points, depending on the particular outcome and comparison group.

Finally, I conduct power calculations on outcomes related to respondents' beliefs regarding social norms, namely, the number of conservative social norms to which the male respondent believes women should adhere, the probability that the male respondent thinks the community ever justifies intimate partner violence, and two indices: one which aggregates the male respondent's agreement with seven conservative social norms, and the other which aggregates the male respondent's second order beliefs regarding the community's sanction of intimate partner violence in seven scenarios. I can detect changes as small as a 0.91 increase in the number of conservative social norms to which a husband adheres, a 0.14 percentage point increase in the probability that a husband believes that the community ever justifies intimate partner violence, a 0.73 standard deviation increase in a husband's adherence to conservative social norms, and a one standard

deviation increase in a husband's belief of the community's sanction of intimate partner violence.

The power calculation above are only pairwise comparisons; the difference in differences specification I employ may allow me to obtain more precise estimates of smaller changes depending on the difference in outcomes between households where men get a job offer and households where women get a job offer. I also hope that more effective enumeration will allow me to survey more pairs of spouses, increasing the endline sample size, and allowing me to detect smaller changes in outcomes with precision.

2.5.4 Heterogeneous Effects

I expect there to be heterogeneity in the effect of a job offer on intimate partner violence based on respondents' characteristics. As mentioned in section [2.5.1](#), heterogeneous impacts of treatment can also help identify the underlying motivations for intimate partner violence in our sample. The main dimensions of treatment heterogeneity I consider are baseline household wealth and the male respondent's baseline adherence to conservative norms. If the impact of job offers to female candidates varies by the male respondent's baseline adherence to conservative social norms, or by baseline household wealth, that can help distinguish between expressive violence as a reaction to women violating social norms, or expressive violence as a reaction to the stress of poverty.

Other important dimensions of heterogeneity from baseline characteristics may be whether female respondents have prior work experience, and whether the respondent and their spouse were able to maximize household payoffs in the first incentivized decisionmaking activity.

If women have already worked outside the home, spouses may have already found a new

equilibrium in household decisions, obviating the need for additional instrumental violence in the bargaining process. Households where women are entering the labor force for the first time may experience more frictions. Households which are able to maximize transfers in the incentivized decisionmaking activity may have fewer conflicts, and may be more able to adapt to women entering the labor force.

When estimating treatment effects in the presence of heterogeneity, I plan to fully interact the characteristic with the initial specification (ie, a triple difference-in-differences specification). This will take the form:

$$\begin{aligned}
Y_{ia} = & \beta_1 + \beta_2 \times \text{Offered Job}_{ia} + \beta_3 \times \text{Female Candidate}_{ia} + \beta_4 \times \text{Characteristic}_{ia} + \\
& \beta_5 \times \text{Offered Job} \times \text{Female Candidate}_{ia} + \beta_6 \times \text{Offered Job} \times \text{Characteristic}_{ia} + \\
& \beta_7 \times \text{Characteristic} \times \text{Female Candidate}_{ia} + \beta_8 \times \text{Offered Job} \times \text{Female Candidate} \times \text{Characteristic}_{ia} \\
& + \mathbb{X}\beta + \epsilon_a
\end{aligned}$$

In this specification, the difference between β_8 and β_5 should reflect how treatment effects vary by a particular characteristic, for women offered jobs. These analyses are likely underpowered.

2.6 Conclusion

Women's employment can be extremely beneficial to households, but may also increase women's experiences of intimate partner violence in the short run. I leverage random variation in the allocation of job offers from a larger experiment to study the impact of a job offer to work

in a mobile money shop on women's experiences of intimate partner violence. In addition to household characteristics, I collect detailed data on the incidence of intimate partner violence, and have respondents participate in two incentivized decisionmaking activities to measure allocation frictions within the household.

The intervention will begin in mid-2023; in addition to collecting endline survey data 6 months after job offers are made, enumerators will also visit respondents employed through the study on a monthly basis to collect data on their attendance, job performance, and reasons for attrition. I hope to use these data to understand the underlying motivations behind intimate partner violence in my sample.

2.7 Tables & Figures

Table 2.1: Female Respondent Characteristics

	Married Female Candidate	Married to Male Candidate
Age	27.53 (7.063)	28.11 (8.535)
Work outside home	.342 (.475)	.207 (.407)
Has NID	.776 (.418)	.714 (.454)
Has phone	.9 (.3)	.652 (.479)
Has smartphone	.646 (.479)	.603 (.493)
Has own SIM	.478 (.5)	.301 (.462)
Has bKash account	.491 (.501)	.198 (.401)
Searching for job	.667 (.472)	.143 (.351)
Has had <5 jobs	1 (0)	1 (0)
Modern lifestyle	.261 (.44)	.216 (.414)
Religious conservative lifestyle	.679 (.468)	.739 (.441)
Traditional conservative lifestyle	.061 (.239)	.045 (.208)
Mother ever worked for pay	.143 (.35)	.099 (.3)
Mother worked outside home	.131 (.338)	.09 (.288)
Mother in law ever worked for pay	.102 (.304)	.063 (.244)
Mother in law worked outside home	.071 (.258)	.063 (.244)
Ever used contraception	.851 (.356)	.794 (.406)
Years married	10.02 (7.625)	10.35 (8.666)

Sample of 238 female candidates and 128 wives of male candidates. Sample averages with standard deviation in parentheses below.

Table 2.2: Male Respondent Characteristics

	Married to Female Candidate	Married Male Candidate
Age	37.5 (12.03)	35.39 (8.807)
Work outside home	.827 (.379)	.89 (.314)
Has NID	.928 (.259)	.968 (.176)
Has phone	.957 (.204)	.992 (.089)
Has smartphone	.534 (.501)	.544 (.5)
Has own SIM	.827 (.38)	.888 (.317)
Has bKash account	.683 (.467)	.79 (.409)
Searching for job	.317 (.467)	.802 (.4)
Has had <5 jobs	.965 (.185)	.973 (.161)
Modern lifestyle	.36 (.482)	.24 (.429)
Religious conservative lifestyle	.596 (.493)	.656 (.477)
Traditional conservative lifestyle	.044 (.206)	.104 (.306)
Mother ever worked for pay	.094 (.293)	.08 (.272)
Mother worked outside home	.08 (.272)	.064 (.246)
Mother in law ever worked for pay	.083 (.276)	.069 (.254)
Mother in law worked outside home	.083 (.276)	.052 (.222)

Sample of 158 husbands of female candidates, 128 male candidates. Sample averages with standard deviation in parentheses below.

Table 2.3: Female Respondent IPV by Candidate Status

	Mean	SD	N
Any IPV ever			
Married Female Candidate, Spouse in HH	.77	.422	204
Wife of Male Candidate, Spouse in HH	.854	.354	103
Any IPV past 6mths			
Married Female Candidate, Spouse in HH	.732	.444	198
Wife of Male Candidate, Spouse in HH	.822	.385	101
Any physical violence ever			
Married Female Candidate, Spouse in HH	.325	.47	197
Wife of Male Candidate, Spouse in HH	.276	.449	98
Any physical violence past 6mths			
Married Female Candidate, Spouse in HH	.194	.397	165
Wife of Male Candidate, Spouse in HH	.155	.364	84

Sample of 238 female candidates, 128 wives of male candidates. IPV includes emotional, verbal, and physical abuse; physical violence restricts to physical abuse. All information on IPV is asked about current marriage. Sample averages with standard deviation in parentheses below.

Table 2.4: Female Respondent IPV by Work Status

	Married Female Candidate		Married to Male Candidate	
	Work for Pay	No Work	Work for Pay	No Work
Any IPV ever	.808 (.396)	.748 (.436)	.87 (.344)	.848 (.361)
Any IPV past 6 mths	.757 (.432)	.719 (.451)	.818 (.395)	.821 (.386)
Any physical violence ever	.429 (.498)	.268 (.445)	.273 (.456)	.28 (.452)
Any physical violence past 6 mths	.259 (.442)	.162 (.37)	.158 (.375)	.156 (.366)

Sample of 238 female candidates and 128 wives of male candidates. 34% of female candidates in labor force at time of survey, 20% of wives of male candidates in labor force at time of survey. IPV includes emotional, verbal, and physical abuse; physical violence restricts to physical abuse. All information on IPV is asked about current marriage. Sample averages with standard deviation in parentheses below.

Table 2.5: IPV Survey Measures vs List Randomization

	Self Report	List Randomization
Married Female Candidate, Spouse in HH	.274 (.447)	.236 (.138)
N	197	219
Wife of Male Candidate	.194 (.397)	.177 (.185)
N	98	106

Sample of 238 female candidates, 128 wives of male candidates. List randomization activity protocol available in appendix B.3. Sample averages with standard deviation in parentheses below.

Table 2.6: IPV Perceptions

	Female Candidate Household		Male Candidate Household	
	Female Candidate [N=238]	Husband of Female Candidate [N=158]	Wife of Male Candidate [N=128]	Male Candidate [N=128]
% Respondents Agree				
A good wife obeys her husband even if she disagrees.	.964 (.186)	.982 (.132)	.99 (.102)	.965 (.184)
Family problems should only be discussed with people in the family.	.909 (.289)	.991 (.094)	.979 (.143)	.957 (.205)
It is important for a man to show his wife/partner who is the boss.	.589 (.493)	.85 (.359)	.784 (.414)	.887 (.318)
If a woman earns more money than her husband, it's almost certain to cause probl	.449 (.499)	.522 (.502)	.485 (.502)	.703 (.459)
It's a wife's obligation to have sex with her husband even if she doesn't feel l	.457 (.499)	.266 (.444)	.464 (.501)	.269 (.445)
If a man mistreats his wife, others outside of the family should intervene	.342 (.476)	.321 (.469)	.26 (.441)	.298 (.46)
% Respondents Believe Community Justifies IPV If				
she goes out without telling him?	.183 (.387)	.196 (.399)	.234 (.425)	.164 (.372)
she neglects the house or the children?	.259 (.439)	.351 (.48)	.254 (.437)	.223 (.418)
she argues with him?	.276 (.448)	.309 (.465)	.241 (.43)	.198 (.4)
she refuses to have sex with him?	.071 (.258)	.124 (.331)	.092 (.29)	.036 (.187)
she doesn't cook food properly?	.061 (.24)	.072 (.26)	.07 (.257)	.1 (.301)
he suspects her of being unfaithful?	.112 (.316)	.052 (.222)	.128 (.336)	.145 (.354)
she shows disrespect for in-laws?	.311 (.464)	.381 (.488)	.315 (.467)	.303 (.462)
Fraction of respondents that report agreement with statement, standard deviation in parentheses below.				

Table 2.7: Incentivized Decision Making Activity Summary

	Female Candidate Household		Male Candidate Household	
	Female Candidate	Husband of Female Candidate	Wife of Male Candidate	Male Candidate
=1 if chose self first	.496 (.502)	.525 (.501)	.411 (.494)	.631 (.485)
IDM1 amt gave up	29.5 (38.08)	27.63 (37.66)	26.88 (37.56)	33.6 (39.88)
IDM1 gave up 0	.453 (.5)	.496 (.502)	.491 (.502)	.42 (.496)
IDM1 gave up all	.259 (.44)	.245 (.431)	.241 (.43)	.304 (.462)
IDM2 amt self	51.58 (33.67)	46.19 (35.31)	41.88 (33.65)	57.12 (34.36)
IDM2 amt spouse	58.42 (33.67)	64.28 (35.02)	68.13 (33.65)	52.36 (34.08)
IDM2 gave all spouse	.129 (.337)	.203 (.404)	.205 (.406)	.1 (.301)
IDM2 kept all self	.115 (.32)	.058 (.235)	.054 (.226)	.136 (.345)

Sample of 139 female candidates, 139 husbands of female candidates, 112 wives of male candidates, 112 male candidates. Protocol for IDM available in appendix B.1 and appendix B.2. Sample averages with standard deviation in parentheses below.

Table 2.8: Characteristics of Respondents by IDM1 Behavior

	Female Candidate Household				Male Candidate Household			
	Female Candidate		Husband of Female Candidate		Wife of Male Candidate		Male Candidate	
	Maximized	Gave Up All	Maximized	Gave Up All	Maximized	Gave Up All	Maximized	Gave Up All
	107	57	69	34	55	27	53	41
Age	27.95 (7.479)	27.68 (7.593)	36.59 (10.96)	35.5 (10.8)	28.62 (8.573)	25.48 (7.787)	35.76 (9.812)	35.54 (8.459)
Work outside home	.383 (.488)	.298 (.462)	.812 (.394)	.794 (.41)	.148 (.359)	.296 (.465)	.925 (.267)	.854 (.358)
=1 if chose self first	.533 (.501)	.491 (.504)	.623 (.488)	.471 (.507)	.527 (.504)	.259 (.447)	.679 (.471)	.683 (.471)
IDM2 amt self	51.78 (29.36)	52.81 (42.92)	47.25 (34.68)	35.88 (35)	49.27 (36)	21.85 (27.74)	59.62 (34.53)	61.95 (37.9)
# conservative norms agrees	3.888 (2.263)	3.719 (2.359)	4.159 (2.2)	4.029 (2.022)	4.455 (2.053)	3.778 (2.259)	4.34 (2.253)	4.805 (1.616)
Believes community ever justifies IPV	.393 (.491)	.386 (.491)	.319 (.469)	.441 (.504)	.582 (.498)	.333 (.48)	.302 (.463)	.439 (.502)
Any IPV ever	.711 (.456)	.787 (.414)	0 (0)	0 (0)	.882 (.325)	.783 (.422)	0 (0)	0 (0)
Any IPV past 6 mths	.655 (.478)	.778 (.42)	0 (0)	0 (0)	.863 (.348)	.773 (.429)	0 (0)	0 (0)
Any physical violence ever	.292 (.457)	.341 (.479)	0 (0)	0 (0)	.3 (.463)	.19 (.402)	0 (0)	0 (0)
Any physical violence past 6 mths	.125 (.333)	.256 (.442)	0 (0)	0 (0)	.146 (.358)	.105 (.315)	0 (0)	0 (0)

Protocol for IDM available in appendix B.1 and appendix B.2. Sample averages with standard deviation in parentheses below.

Table 2.9: Incentivized Decision Making Activity by Initial IDM1 Choice

	Female Candidate Household			Male Candidate Household		
	Female Candidate Self	Female Candidate Spouse	Husband of Female Candidate Self	Wife of Male Candidate Self	Male Candidate Self	Male Candidate Spouse
IDM 1 Initial	.496 (.5)	.504 (.5)	.525 (.499)	.414 (.5)	.646 (.499)	.475 (.499)
IDM2 kept all self	.087 (.285)	.031 (.174)	.16 (.369)	.414 (.5)	.646 (.499)	.475 (.499)
IDM2 gave all spouse	.109 (.315)	.277 (.451)	.037 (.19)	.2 (.405)	.646 (.499)	.475 (.499)
IDM2 amt self	53.48 (32.88)	33.54 (32.18)	67.07 (30.73)	43.11 (36.11)	.646 (.499)	.475 (.499)

Sample of 139 female candidates, 139 husbands of female candidates, 112 wives of male candidates, 112 male candidates. Protocol for IDM available in appendix B.1 and appendix B.2. Sample averages with standard deviation in parentheses below.

Table 2.10: Differences in Norms, Cooperation by Candidate Gender

	Female Candidate Household	Male Candidate Household
Both respondents choose husband IDM1 initial	.164 (.371)	.289 (.455)
Amt both respondents give to husband IDM2	104.6 (51.14)	125.3 (47.67)
# conservative norms husband agrees	3.551 (2.412)	4.492 (1.944)
Index husband conservative norms	-.007 (1.165)	-.042 (1.772)
Husband thinks community ever justifies IPV	.342 (.476)	.414 (.494)
Index husband beliefs community IPV	-.022 (1.57)	.023 (1.748)

IPV sample of 238 female candidate households, 128 male candidate households; IDM related sample of 139 female candidate households, 112 male candidate households. Sample averages with standard deviations in parentheses below.

Table 2.11: Power Calculations

Potential Employee Comparison	# Female Candidate Job Offer	# Comparison Group	# Treated Shops	# Control Shops	ICC	Baseline Mean	MDE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any IPV past 6 mths	Female w/o offer	98	12	49	.249	.738	.315
	Male w/offer	29	12	15	.249	.738	.38
	All	292	12	146	.249	.738	.294
Any physical violence past 6 mths	Female w/o offer	71	9	36	.264	.146	.295
	Male w/offer	29	9	15	.264	.146	.335
	All	265	9	133	.264	.146	.272
Husband choose self IDM1 initial	Female w/o offer	258	32	129	.128	.444	.207
	Male w/offer	29	32	15	.128	.444	.331
	All	452	32	226	.128	.444	.198
Husband amt give up IDM1	Female w/o offer	258	32	129	.099	29.41	15.67
	Male w/offer	29	32	15	.099	29.41	25.07
	All	452	32	226	.099	29.41	14.99
Husband maximize transfer IDM1	Female w/o offer	258	32	129	.125	.463	.207
	Male w/offer	29	32	15	.125	.463	.331
	All	452	32	226	.125	.463	.198
Husband give up all IDM1	Female w/o offer	258	32	129	.14	.264	.184
	Male w/offer	29	32	15	.14	.264	.295
	All	452	32	226	.14	.264	.176
Husband allocate self IDM2	Female w/o offer	258	32	129	.051	44.47	13.92
	Male w/offer	29	32	15	.051	44.47	22.28
	All	452	32	226	.051	44.47	13.31
# conservative norms husband agrees	Female w/o offer	258	32	129	.033	1.699	.962
	Male w/offer	29	32	15	.033	1.699	1.54
	All	452	32	226	.033	1.699	.92
Husband thinks community ever justifies IPV	Female w/o offer	258	32	129	.048	.168	.15
	Male w/offer	29	32	15	.048	.168	.24
	All	452	32	226	.048	.168	.143
Index husband conservative norms	Female w/o offer	86	11	43	0	-.026	.795
	Male w/offer	29	11	15	0	-.026	.936

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	Potential Employee Comparison	# Female Candidate Job Offer	# Comparison Group	# Treated Shops	# Control Shops	ICC	Baseline Mean	MDE
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	21	280	11	140	0	-.026	.739
	Female w/o offer	20	82	10	41	.117	-.011	1.163
	Male w/offer	20	29	10	15	.117	-.011	1.353
	All	20	276	10	138	.117	-.011	1.08
Index husband beliefs community IPV								

Table 2.12: Pattern of Effects: Female Candidate Receives Job Offer

Hypothesized Channel	β_4 , Takeup	β_4 , IPV	β_4 IDM Self, Men	β_4 Norms, Men
Expressive Violence, Poverty	+	-	?	?
Expressive Violence, Violation of Modesty Norms	-	+	?	+
Instrumental, Conflict over Surplus	+	+	+	?
Instrumental, Women's Outside Option Improves	+	-	?	?

Chapter 3: The Effect of an Exogenous Shock to Labor Demand on Household Composition and Perceived Returns to Education

3.1 Introduction

In Bangladesh, migration is one of the many tools poor households use to smooth consumption and increase income. However, migration brings with it additional risk and uncertainty for households which send migrants. Migrant household members send remittances that increase overall household income, which can help origin households smooth against local shocks. However, there is a high fixed cost to migration, and uncertainty about whether migrants will find work when they arrive at their destinations. Migrants themselves are exposed to an independent set of shocks, for which they may require support from their origin households. Household decisions to send a migrant member, or migrant decisions to remain away from the household, may change over time as employment conditions change.

In this paper, I consider internal female migrants, who comprise a major portion of the workforce in Bangladesh's ready made garments (RMG) sector. While the level of migration implied by my data is small, it occurs in an economically significant sector: the RMG sector comprises 83% of Bangladesh's export earnings. I study the impact of an exogenous negative shock to the employment of female migrants—a building collapse in Savar, Dhaka, which prompted the

closure of a large number of firms in the export oriented RMG sector. I use a difference in differences specification to compare outcomes in households within districts which have a strong history of sending women to work in factories (“sending districts”), with their counterparts in districts that do not send female migrants (“non-sending districts”), before and after the collapse.

I find an increase in unmarried working-age women living in sending districts in the aftermath of the collapse. This suggests that migrant women responded to the initial shock and returned to their households of origin rather than remain in Dhaka, or that the shock inhibited migration and households did not send women to work in factories as they would have prior to the collapse. Given the high fixed costs of migration, this could suggest that households initially believed the shock would persist for a long time and the cost of women remaining unemployed away from home was higher than the cost of return migration. Alternatively, women may have had better labor market opportunities in their districts of origin, and the opportunity cost of remaining in Dhaka was too high. I find that household composition reverted to pre-shock levels by 2018, three years after the factory collapse. I interpret this as working age women returning to garment factory work, as the sector had largely recovered by that point.

I also find a small increase in the probability that households in sending districts have a daughters-in-law after the shock, suggesting that some fraction of women whose employment opportunities were affected by the shock turned to marriage in lieu of migration, or upon their premature return from working in garment factories after the shock.

I also look at the impact of return migration on rural labor markets. Unskilled agricultural labor forms the majority of paid work available to women in rural Bangladesh. If return migration caused a positive labor supply shock, wages for unskilled agricultural labor would presumably fall in the short term. I see no changes in the daily agricultural wage rate for women in the year

after the shock. This may be because the increase in labor availability due to return migration or inhibited migration caused by the factory collapse was too small to impact wages. It also may be the result of a variety of frictions in the market for agricultural wage work. This does not rule out the possibility that potential migrants worked as casual agricultural laborers upon their early return, or in lieu of migration.

Lastly, I look at the impact of the shock on children's education. Garment factory jobs, unlike unskilled agricultural labor, require some education. If households perceived garment factory job losses to be permanent, the shock may have changed their beliefs regarding the returns to education for girls. The loss of remittances from garment workers may also have affected households' ability to send children to school, or increased the opportunity cost of a child remaining in school. I see no decrease in the fraction of school aged children enrolled, nor delays in initial school enrollment of 5 year old children; in fact, there is a small increase in the initial enrollment of 5 year old boys. I interpret these findings as evidence that households either correctly anticipated the garment sector would return to normal, that households valued children's school attendance for other reasons, and/or that households had access to tools to smooth consumption, including income earned by returned or inhibited female migrants.

The remainder of this paper is as follows: section 3.2 describes the context of women's migration and labor force participation in Bangladesh, and the impact of the Rana Plaza factory collapse on the ready made garment industry. Section 3.3 describes the data sources I use in my analysis, and section 3.4 describes the identification strategy. Section 3.5 presents results, and section 3.6 concludes.

3.2 Background

3.2.1 Female Labor Force Participation

36% of working age women participate in the labor force in Bangladesh (World Bank, 2019). This is similar to other countries in South Asia, and is likely a result of strict social norms regarding women's roles in society [60]. Women in Bangladesh have few opportunities to work for pay outside the home. Acceptable semi-skilled or skilled jobs for women include tailoring, teaching, or working in a ready made garment (RMG) factory [25]. Alternatively, women may work in unskilled agricultural labor, performing gender-specific tasks which are compensated at a lower rate than work performed by men.

Working in a high skilled occupation not only provides income, but also has non-pecuniary benefits. Buchmann et al. [25] use a vignette based exercise with matchmakers across Bangladesh to study factors that influence women's marriage market prospects. The authors find that working in skilled jobs before marriage improves match quality, reduces dowry payments, and increases the amount of denmeher (payment to the bride from the groom's household). This holds true in other parts of South Asia. Sivasankaran [96] provides empirical evidence from Tamil Nadu using a sample of unmarried migrant women who were employed at a textile mill before marriage. She exploits exogenous variation in contract length and finds that women see no decrease in marriage quality (number of offers, value of gifts exchanged before marriage, spouse characteristics), and have higher levels of empowerment and delayed fertility the longer they work. Jensen [61] provides experimental evidence on the benefits of skilled, white-collar work on investments in, and outcomes for, young women in rural north India. Jensen randomly provides women assistance

in applying for business processing outsourcing jobs, and finds that households in the treatment group were more likely to invest in girls' human capital and delay girls' marriage, while girls in the treatment group expressed desire for lower total fertility.

The presence of skilled employment opportunities can also affect households' perceptions of the returns to education for children who may be able to take advantage of these jobs in the future. Households may of course value children's education for other reasons, but an exogenous increase in the availability of well paying skilled jobs could encourage marginal households to invest in children's education in the absence of significant market failures. Several studies have found that an increase in the availability of skilled jobs for women increases education levels of girls who are too young to work, but who may work in the future. Heath and Mobarak [51] find that an increase in the number of garment factories near a household increases investment in education for young girls in Bangladesh relative to boys, but has no impact in villages far away from the factories. Oster and Steinberg [79] use a similar approach and find that girls' enrollment in schools located near Information Technology Enabled Service Firms (ITES)—which prefer to employ young women over young men—increased in the years after ITES firms arrived in the area. While Heath and Mobarak [51] and Oster and Steinberg [79]'s work considers the effects of physically proximate jobs, further work by Boudreau et al. [23] shows that workers rely primarily on referrals from extended social networks to get jobs. As such, it's plausible that education would increase for girls who live far away from factories, if their households have strong ties to current factory workers. I use a proxy of such social proximity—whether a district has a history of sending migrants to work in garment factories—in my analysis.

3.2.2 Garment Factories in Bangladesh & Rana Plaza Collapse

Ready made garment (RMG) factories in Bangladesh are concentrated around the capital of Dhaka, with a smaller number located in Chittagong division. Factories tend to cluster in industrial areas as well as special export processing zones, which provide financial incentives to export oriented firms. In addition to larger, export-oriented factories with direct relationships to international brands, many exporting factories subcontract to smaller firms which are not subject to the same level of scrutiny from their clients. Multiple, unrelated firms often operate out of a single factory building.

Garment factories in Bangladesh employ mostly unmarried women between the ages of 14 and 20 who migrate to Dhaka and Chittagong from rural areas specifically to work in garment factories. Women usually find jobs through established networks of previous migrants from their villages, as in many other contexts [19, 23, 24, 75]. Women maintain ties with their origin households, and may visit during festivals or other important occasions. After working for a few years, most women return to their homes to get married and leave the labor force [25].

Rana Plaza, a building that housed several RMG factories, collapsed on 24 April 2013. The official death toll released by the Bangladeshi government was 1,134, mostly young women [66]. Many more workers were injured in the collapse. The building had a history of safety violations, including a fire a few months before the collapse, and been subject to complaints regarding large, visible cracks in the building's foundation. In addition to structural issues, the owners neglected to install basic safety features such as working fire doors, emergency exits, fire alarms, sprinklers, which all would have substantially improved working conditions in the factory.

The international community reacted swiftly to this disaster. Two groups were formed by

international brands to ensure worker safety: the Alliance for Bangladesh Worker Safety (the Alliance) and the Accord on Building and Fire Safety (the Accord). These groups worked with the government of Bangladesh to inspect RMG factory buildings for worker safety, and assisted building owners in remediating poor working conditions. However, both groups only committed to these efforts for a period of five years. After 2018, the government of Bangladesh stated its intent to independently continue inspections, closures, and remediation.

The extent to which the RMG sector was affected by these regulatory bodies is unclear. All export oriented RMG factories were in theory subject to inspections, and could not resume operations without meeting basic safety standards. However, the size of the sector was, and still is, unknown to both the government and international brands. Some RMG factories likely escaped inspections, especially if they were subcontractors to larger exporting firms. Official figures reported by the government show that 1,549 firms were inspected, of which 513 were closed due to unsafe conditions [86]. Information from the Alliance shows that of 714 participating factories, 149 were forced to suspend operations. Data from the Accord shows that of 1,664 factories covered, 152 were suspended. These figures do not include subcontractors, nor do they include factories not eligible under the Accords (ie factories that did not supply to the brands that were party to the Accord and the Alliance). Even so, the policies implemented in the aftermath of the collapse did force some firms to close permanently, and others to close temporarily. If a factory shut down, a new operator could not immediately open in the same building; fixing structural issues took time and money. Some jobs were certainly eliminated in the aftermath of the collapse.

Two pieces of data provide evidence that employment opportunities in the RMG sector fell in the immediate aftermath of the Rana Plaza collapse. First, the Bangladesh Garment Manufacturers Export Association (BGMEA), which purports to cover 70% of all export oriented

RMG factories, published data showing the number of factories with membership in the association each year [20]. Figure 3.1 shows a dip in the number of factories in 2013 and 2014, likely attributable to the inspections and shutdowns in the aftermath of the collapse.

Second, as part of the larger Ready Made Garments Productivity Project, Woodruff and Miyata [102] published data which describes the characteristics of the factories participating in the study. Using administrative data, the authors show that the rate at which employees left garment factory jobs in May 2013 was much higher than the rate in the same month during any subsequent year. Figure 3.2 illustrates this rise in migration, which is attributed to the Rana Plaza collapse.

Using the migration rates in figure 3.2, I perform a permutation test. Out of the $20!$ possible assignments of migration rates to months, only one assignment—the observed assignment—assigns the highest migration rate to May 2013. The probability of this assignment being random is 4×10^{-19} .

However, the factory leaving rate in July 2014 is much higher than other months. This may be due to the fact that Eid-Al-Fitur took place in July in 2014. At the same time, we do not see similar increases around Eid in 2012 or 2015. Further, the leaving rate in May 2012 was higher than the leaving rate in any other month of 2012, which I also cannot explain. This evidence should be interpreted with caution, but does suggest the Rana Plaza collapse and associated inspections had negative consequences for employment levels in the garment sector.

3.2.3 Hypotheses

Ex ante, the impact of an exogenous decrease in garment factory jobs on migration decisions is ambiguous. Households consider two sets of outcomes when deciding whether to support a young female migrant: the immediate costs and benefits (cost of travel and support before she finds a job, the cost of supporting her if she faces a negative income shock, the income she can send to her family if she obtains a job), and the long term benefits (improved groom quality, more favorable payments at marriage, better health due to delayed childbearing). Families may choose to send women to migrate even if the short term pecuniary benefits are negative in expectation, as long as the net benefits outweigh the short and long term costs. Similarly, if the costs of return migration are high, families may choose to provide financial support to migrant women facing transitory negative employment shocks, rather than facilitating their return home. As such, it is unclear whether women will return to their origin households, increasing the number of working age women in residence in sending districts compared to non-sending districts, or whether women will stay in Dhaka, leaving the number of working age women in residence unchanged. If we find that the number of working age women in residence in sending districts *decreased* in the aftermath of the collapse, it would cast doubt on whether the identification strategy captures the effect of the negative labor demand shock due to the factory collapse.

Returning migrant garment workers have limited opportunities at home. Marriage is a common alternative to labor force participation, and most Bangladeshi women leave the formal labor force upon marriage. I am under powered to test changes in the rate of new household formation, and also lack data on date of marriage. However, marriage in Bangladesh is patrilocal—women typically reside with their husbands’ extended families upon marriage. I can look at the

number of daughters-in-law in residence in sending district households compared to non-sending district households, before and after the shock. An increase would indicate that some fraction of women respond to employment loss (or preemption of employment) by marrying. If marriages occur between sending and non-sending districts, it would attenuate my results to zero. Finally, if some marriages result in the formation of new households rather than living with in-laws in a joint household, this would also attenuate my results to zero.

If women do remain in the labor market, unskilled casual agricultural labor is the most common form of female employment in their districts of origin in rural Bangladesh. Women perform specific tasks that are labor intensive rather than capital intensive, and which are compensated at a lower rate than work performed by men. Even when women perform the same tasks as men, they are compensated at a lower rate [40]. In the absence of labor market failures, a sufficiently large exogenous increase in the supply of working women in rural areas would lower the equilibrium market wage. If the magnitude of the supply shock is not sufficiently large, or if wages are rigid—due to the presence of nutritional efficiency wages [99], fairness norms [64], or because production decisions had already been made before the shock—we might not see changes in the prevailing wage rate. These labor market frictions would instead cause an increase in the level of unemployment, a change in the composition of workers, a change in the level of labor supply to own household farms, or some combination thereof. I am limited to looking at changes in the daily wage rate, as I lack data on employment, labor force composition, and household labor supply. A negative or null result on wages would be consistent with the above hypotheses. If instead, we see an increase in the equilibrium wage, this would imply that the identification strategy is not picking up the effect of the factory collapse on wages.

Whether or not households change their investments in children in response to an exoge-

nous change in *current* gender-specific skilled employment opportunities depends on whether they value their children's education independently of its return on the job market, whether education has returns outside the job market, whether they believe the shock to employment to be permanent, and the constraints they face in financing their children's education. For example, if factories which employ women suddenly close, households may still continue to invest in their daughters' education if the cost of an additional year of education is less than the benefits, even if those benefits are realized in the marriage market rather than the labor force. Parents may choose to delay their investments until uncertainty in the job market is realized. While this may reduce the number of years that young women study or work (if the age of marriage does not also shift and is a binding constraint), it may be less costly in expectation than investing in children when results are uncertain. However, it is more likely that education decisions on the margin involve older children who have more labor market opportunities than younger children, increasing the opportunity cost of their education. Older children in sending districts may leave school earlier than they would have in the absence of the shock in order to take on paid employment and make up for lost remittance income.

3.3 Data

I use four separate sources of data in my analysis. To determine the districts of origin of female migrant workers, I use data from a survey of garment workers conducted in Dhaka in 2009 by Heath and Mobarak [51]. I rank districts in decreasing order by the number of migrants they send, and define sending districts as the top districts that send 80% of the migrants in the dataset. This cutoff as it represents all districts which send more than one unmarried female

migrant worker to work in garment factories. In robustness checks, I look at districts that different proportions of garment workers report as their home districts in the Heath and Mobarak [51] data. I perform this exercise for my main outcome of interest, the probability of having at least 1 woman age 14-20 living in the household. These results are available on request

I use household survey data from the Demographic and Health Surveys (DHS), collected in 1999, 2004, 2007, 2011, 2014, and 2018. This survey collects detailed household roster information which I use to estimate changes in household composition and investments in human capital as a result of the factory collapse. The DHS is undertaken to provide information on women's and children's health in Bangladesh, and is nationally representative. These data include each household's location represented as GPS coordinates which are randomly offset by 2 to 5 km; 10% of coordinates offset up to 10 km. Even with this random offset, I can correctly identify the district in which a household is located.

I use data on agricultural wages produced by the Bangladesh Bureau of Statistics in its Yearbook of Agricultural Statistics. Nominal wage data (both cash payments and the value of in-kind payments) is collected by district statistical offices and reported to the Bureau on a monthly basis. Until the end of 2013, data was published at the *greater district* level. This is a historic administrative division which has 21 units representing the country's organization when Bangladesh was still part of Pakistan. Each greater district can be mapped to between 1 and 4 current districts; no current districts belong to more than one greater district. I aggregate wage data published after 2013 to the greater district level based on these mappings.

I collect data on inflation from the Bangladesh Bureau of Statistics, published in its Monthly Statistical Bulletin. This publication collects information on the price of a bundle of goods (separate bundles for rural and urban areas) on a monthly basis in a predetermined set of regions across

the country. I use changes in the rural CPI to calculate the real agricultural wage.

My final sample for regressions on household composition and education regressions contains 84,254 households in 64 districts, of which 14 are sending districts, from 1999 to 2018. My final sample for regressions on wages has monthly data for 21 greater districts (14 sending districts) over 18 years.

3.4 Identification Strategy

My identification strategy relies on the assumption that in the absence of the factory collapse, the value of the outcomes I study would have continued along the same trends they had followed prior to 2013. I use a difference-in-differences strategy to compare the number of working age women in households between “sending” districts (districts from which women migrate to go work in garment factories) and non-”sending” districts before and after the factory collapse.

$$Y_{dut} = \beta_1 + \sum_{t=2000}^{2016} \beta_t \times Year_t + \beta_3 \times Sending_d + \sum_{t=2000}^{2016} \alpha_t \times Year_t \times Sending_d + \mathbb{X} + \epsilon_u$$

where Y_{dut} is the outcome for a household residing in union u within district d at time t . $\mathbb{X}$ is a vector of controls including region fixed effects, $\sum_{t=2000}^{2016} Year_t$ is a set of indicator variables for the year the survey took place with the omitted category as year 1999, and ϵ_u is an error term. In my preferred specification, I cluster standard errors at the union level.

In addition to examining the impact of the factory collapse on the number of unmarried,

working-age women within a household, I use this specification to examine the impact of the factory collapse on the number of unmarried, working age women (14-20) in the household, and the probability of having at least one daughter in law in the household. I also look at the probability of having a son-in-law in the household. However, since sons are more likely to live with their parents in joint families, or alone, rather than with their in-laws, I should not see any changes in number of sons-in-law in the household. The final outcomes I consider using this specification are school enrollment rates between sending and non-sending districts before and after the factory collapse, to see whether or not the factory collapse affected perceived returns to schooling for girls or boys, or the opportunity cost of remaining in school.

My preferred specifications include controls for the age difference between the head of household and spouse, education levels of the head of household and spouse, household religion, and cluster standard errors at the union level, the administrative division one level below the district.¹

I also study the impact of the factory collapse—and associate decrease in skilled employment opportunities—on the agricultural wage. In this specification, the dependent variable will be the nominal or real daily agricultural wage rate (average over the month). However, in this case the data is identified at the great-district month level rather than the individual level. I include month fixed effects and region fixed effects in this specification, and cluster results within regions. I present results for the daily nominal and real wage rates for unskilled women performing agricultural labor. I also present results for the nominal and real wage rates including the value

¹The level of intracluster correlation at the union level is very low (0.03). Ideally, I would cluster my standard errors at the village or ward level, but the data I have is not identified at that level

of non-monetary compensation.

$$\begin{aligned}
 \text{Daily Agricultural Wage for Women}_{drt} = & \alpha + \sum_{t=2000}^{2016} \beta_t \times \text{Year}_t + \gamma \times \text{Sending}_{dr} \\
 & + \sum_{t=2000}^{2016} \zeta_t \times \text{Year}_t \times \text{Sending}_{dr} + \mathbb{X}\phi + \sum_{m=\text{Jan}}^{\text{Dec}} \tau \times \text{Month}_m + \sum_{r=1}^7 \delta \times \text{Region}_r + \epsilon_r
 \end{aligned}$$

3.5 Results

3.5.1 Summary Statistics & Parallel Trends

I present trend graphs of each outcome variable in figures 3.3 and 3.4. The data collected by Heath and Mobarak [51] show that women come from districts across Bangladesh, but not all districts within a region send female migrants to work in garment factories. There is likely large variation in villages within districts that send women to work in garment factories, but I do not observe this in the data. I do see that there are differences between districts that do send migrants to work in garment factories and those that do not. I present summary statistics by sending and non-sending districts in table 3.1. While there are differences in the levels, I find that *trends* before the shock in 2013 are parallel for outcomes related to household composition. I observe this graphically in figures 3.3 and 3.4, and in tables 3.2 and 3.3.

3.5.2 Minimum Detectable Effects

Between 16.3 and 21.9% of households in Bangladesh have an unmarried woman between the ages of 14 and 20 in residence. Households in districts that have a strong history of sending

female migrants to work in the garment sector have 0.021 fewer unmarried working age women in residence than households in non-sending districts. This implies a migration rate of 10%. Given the relatively low incidence of female migration, and the fact that I use a nationally representative survey, it is likely that I am underpowered to detect small changes in outcomes. I present the size of the smallest effect I can detect as a percentage of the reference group mean in table 3.4 for each outcome of interest that I study. I calculate the minimum detectable effect in two ways: comparing sending districts after the collapse to sending districts before the collapse, or alternatively, comparing sending districts after the collapse to non-sending districts after the collapse. The larger of the two would be the smallest effect I am powered to detect. I present two results for each comparison, first assuming that the shock has a persistent effect over all years (4 years for wages, 5 years for household level outcomes) after the shock, and then assuming the effects of the shock will only be apparent in the year immediately after the shock (1 year). In all calculations, I regress the outcome of interest on year and region fixed effects, in addition to household control variables, restricting the sample to the relevant counterfactual. I conduct the calculations on the resulting predicted values.

My calculations show that given 80% power and a 5% significance level, I should be able to detect between a 6.4 and 20% increase in the number of women aged 14-20 in residence in sending district households. I would only be able to detect 18.4 to 32.4% changes in the probability a sending district household has at least one son or daughter in law in residence. Given that this would be a second order effect—that migrant women would return from factories and then a fraction of those women would marry—it makes sense that I am underpowered to detect small changes. Additionally, since some fraction of marriages would take place between sending and non-sending districts, my results would be an underestimate of the magnitude of the marriage

response. Lastly, my results for the number of unmarried women in residence and the number of daughters-in-law in residence deal with two possible outcomes on a fixed sample of migrants—an increase in one outcome attenuates the result of the other outcome. As such, the absence of a statistically significant result may be a consequence of limited statistical power rather than the shock not having an effect.

For outcomes related to children’s schooling, I am able to detect changes which are between 4.6% to 18.2% as large as the pre-shock mean in non-sending districts. I am powered to detect smaller changes in enrollment rates for children 5 to 14 years old, but can only detect larger effects for enrollment rates for 5 year olds.

I present the smallest effects that I am powered to detect for outcomes related to agricultural wages in table 3.5. I am powered to detect changes between 14.7 and 25.5%, which imply wage elasticities between -0.9 and -1.57. I discuss the implications of these magnitudes in section 3.5.5.

3.5.3 Household Composition

Table 3.6 shows the impact of the factory collapse on household composition. The coefficients of interest are the interaction terms of sending district (an indicator for whether the district historically sent female migrants to work in garment factories), and post (an indicator for whether the survey occurred after the factory collapse).

The first column of table 3.6 considers the number of unmarried woman aged 14-20 who reside in the household. These are women who are in the age range of potential factory workers, and who would be “treated” by the factory collapse. Note that this specification is an “intent to treat” design: I do not observe directly whether these women ever migrated or worked in a

garment factory. However, if I see an increase in the number of women in that age range within the household after the shock, it provides evidence that the shock either inhibited migration to the garment sector or that these women returned prematurely from employment due to the shock.

The indicator for sending districts is negative: sending districts have 0.02 fewer women aged 14-20 in residence, likely a consequence of the fact that they have higher levels of female migration to work in garment factories. I also observe that the number of working aged women is declining over time, both in sending and non-sending districts. However, I observe that there is a 0.024 increase in the number of women between 14 and 20 years old who live in the household, only in sending districts, in the aftermath of the factory collapse. This is approximately a 10% effect size, which is close to what I am powered to detect. These results suggest that nearly all the women who had migrated returned, and that some women would have migrated did not. I also present p-values estimated by wild cluster bootstrap that the coefficient on the interaction term is equal to zero; these p-values show I can reject the null that the interaction is equal to zero.

In column 2, I consider whether the household has a daughter in law in residence. Given that marriage patterns are patrilocal, and that some fraction of women responded to the decrease in labor demand by choosing to marry, any increase in the probability of having a daughter-in-law within a household would provide support for this hypothesis. I find that there is a 2.7 percentage point increase in the probability a household has a daughter-in-law in residence over time in both sending and non-sending districts; however, there is no difference in the probability of having a daughter-in-law in residence between sending and non-sending districts. When I look at the probability of having a daughter-in-law in the household in sending districts after the collapse, I find that this probability increases by 1.3 percentage points. The baseline probability of having a daughter-in-law in the household was 11.8%; the increase that may come from the factory

collapse is approximately a 10% increase, which is smaller than I am comfortably powered to detect. P-values estimated by wild cluster bootstrap testing that the coefficient on the interaction term is equal to zero is 0.009, which implies that this result is statistically significant.

Lastly, in column 3, I estimate of the shock on the probability that a son-in-law lives in the household. Considering again that marriage patterns are patrilocal in Bangladesh, the probability of having a son-in-law in the household should both be very low, and should not change in only sending districts due to the factory collapse. I find in general that the probability of having a son-in-law in the household is declining over time in both sending and non-sending districts, though there is no level difference in this probability between sending and non-sending districts. When I look at the probability of having a son in law in a sending district after the factory collapse, I find that this probability increases by half a percentage point. Given a baseline probability of 0.02 percentage points, this is approximately a 25% increase, well within the size of effects that I have statistical power to detect. This result does suggest that either the factory collapse changed patterns of residence in extended families, or that my identification strategy is capturing changes in household composition attributable to factors other than the factory collapse. The p-values from a wild cluster bootstrap is 0.065, suggesting that this effect is statistically significant.

In table 3.7, I interact indicator variables for the year of survey with an indicator for a district sending female migrants to work in the garment factories. I estimate results on the same outcomes as table 3.6: the number of unmarried women between 14 and 20 years old, the probability of having a daughter-in-law in the household, and the probability of having a son-in-law in the household. If the interaction between the indicator for sending district and the year 2014 are significantly different from the interactions between earlier survey rounds, this could suggest a causal effect of the factory collapse. I present p-values for a test of equality of the interaction

between 2014 and the indicator for a sending district with all other year-sending district interactions, as well as the corresponding p-values for the 2018 round interacted with the dummy for sending district. A statistically significant interaction between the 2018 survey round and an indicator for sending districts could be interpreted as either a persistent or delayed causal effect, depending on whether or not the interaction with the 2014 survey round was statistically and economically significant.

Column 1 of table 3.7 examines the impact of the shock on the number of unmarried women age 14-20 in the household. The indicators for survey year show that the number of women aged 14-20 was generally decreasing over time across both sending and non-sending districts. Sending districts also had a smaller number of women in residence overall, consistent with the fact that these districts send female migrants to work in RMG factories. I observe an increase of 0.03 women in sending districts the year following the factory collapse, and no change in any other year in sending districts. The implied effect size is smaller than what I am powered to detect. No other interaction terms are significant. I present p-values from two independent tests that the interaction terms from 2014 or 2018 are equal to the value of the other survey year interaction terms. I use a wild cluster bootstrap to estimate p-values; the p-value of the first test is 0.22, and the p-value of the second is 0.24, showing that these results are not statistically significant at conventional levels. While these results provide suggestive evidence that the factory collapse inhibited migration or caused an increase in return migration, I cannot reject that the increases in 2014 and 2018 were simply part of an imprecisely estimated differential trend in sending districts.

In column 2, I look at the probability that there is a daughter-in-law in the household. I find that the likelihood of a daughter-in-law living in a joint household is increasing over time in both sending and non-sending districts, and that there is no level difference between sending and non-

sending districts. When I look at the interaction between the dummy for sending district and each survey year, I find that only the interaction for the 2014 survey round is significant: this implies that there was a 0.02 percentage point increase in the probability of having a daughter-in-law in the household only in sending districts in the immediate aftermath of the collapse. No other interaction terms are statistically significant, suggesting that there was no differential pre-trend in sending districts prior to the collapse, nor did this change persist after 2014. However, when I test whether the coefficient on the interaction between the year 2014 and the indicator for whether the household is in a sending district is equal to the coefficient on the remaining interaction terms, using a wild cluster bootstrap to compute standard errors and p-values, I cannot reject that they are part of the same imprecisely estimated trend. Again, these effect sizes are smaller than what I am powered to detect.

In the last column, I check whether the factory collapse had an effect on the probability of having a son-in-law in the household. There does not appear to be any discernible trend over time, nor any difference in the probability of having a son-in-law in residence between sending and non-sending districts. When I look at the interaction terms between year of survey and whether the household lives in a sending district, I find no statistically significant effect of the interaction between the 2014 survey round and the indicator for sending district. However, there is an increase of 0.01 percentage points in the probability of having a son-in-law in the household in sending districts in 2018. When testing that this coefficient is equal to all the other interaction terms, using a wild cluster bootstrap to calculate p-values, I find that I can reject that it is; the p-value is 0.018. This result suggests there may have been other events in the aftermath of the factory collapse that only affected sending districts which changed patterns of marital migration.

3.5.4 Education

The reduction in labor demand for female migrant workers in the aftermath of the Rana Plaza collapse may have affected children's education in districts that traditionally sent migrant workers through several channels. First, the reduction in remittances may have had a direct income effect, and pulled both boys and girls out of school and into the labor force to make up for the shortfall in household income. The same holds true if sending households had to support female migrant workers during their unemployment after the shock. This income effect could be stronger for boys than for girls, given that girls in sending districts have higher returns to education due to known opportunities in the garment sector, while boys' schooling may have a smaller pecuniary return. However, if there are more labor market opportunities for boys, the opportunity cost of continuing schooling is higher, as they are giving up a larger income than their sisters. Finally, if households were unable to borrow or reallocate consumption to make up for lost remittances, they may have chosen to delay initial enrollment for both 5 year old boys and girls, or have older children leave school prematurely to join the labor force.

Alternatively, since the shock affected girls' employment prospects, but not boys', it may have changed parents' expectations of the returns to girls' education. Instead of improving both girls' employment and marriage prospects, education would only improve a girls' marriage prospects if the shock caused a permanent decrease in the number of garment factory jobs. If the direct or indirect costs of schooling were higher than the benefit girls would receive in the marriage market, parents may have chosen to reduce their investment in girls' education. Uncertainty in returns to girls' education may have delayed girls' initial enrollment in schooling. While delaying school enrollment may reduce a girl's education—and potentially, earnings—if age

at marriage is kept constant, which it generally is, parents may still have chosen to wait until the uncertainty was resolved before committing to invest in their daughters' education, rather than undertaking a risky investment. At the same time, if the returned migrants contributed more to the household than the remittances they sent—or if the household saved money by not sending migrants—the household could choose to enroll boys—whose education has a lower rate of return—in school.

As outlined in table 3.4, I am powered to detect small changes in enrollment rates for boys and girls between 5 and 14 years of age (minimum detectable effects of 4.6% to 6.8%), and less well powered to detect effects on the enrollment margin for 5 year old children (minimum detectable effects of 10.5 to 18.2%).

In table 3.8, I first consider a simple specification where I interact an indicator for a district sending female migrants to work in garment factories with an indicator for the survey taking place after the factory collapse. I present results on four outcomes: the number of 5 year old girls living in the household who are enrolled in school, the number of 5 year old boys living in the household who are enrolled in school, the fraction of girls in the household who are age 5 to 14 who are enrolled in school, and the fraction of boys in the household who are age 5 to 14 who are enrolled in school.

In column 1, I examine whether the factory collapse affected the enrollment of girls who were about to start school. I find that school enrollment rates are generally rising, and that there is no difference in the level of school enrollment between sending and non-sending districts. I also do not find a statistically significant change in the number of 5 year old girls enrolled in school after the factory collapse in sending districts. However, the size of the point estimate is smaller than the smallest effect I am powered to detect, so it is possible the true effect is very small in

magnitude. The p-value from using a wild cluster bootstrap to test whether the interaction is significant is 0.44, which means I cannot reject that the true effect is zero.

I find a similar pattern for the number of 5 year old boys enrolled in school: the number is increasing over time in both sending and non-sending districts, with no level difference between the two types of district. I observe an increase of 0.09 boys enrolled in sending districts after the collapse, which is within the range of effect sizes I am powered to detect. The p-value from a wild cluster bootstrap test to assess the significance of the interaction term is 0.12, which means I cannot reject that the true effect is zero.

One explanation for the imprecisely estimated increase in girls' and boys' education I observe is that female migrants who returned home in the aftermath of the shock were able to earn more income than the remittances they sent from the factory. This additional income could have reduced constraints on school enrollment. However, this could also be the result of other circumstances which affected sending districts after the shock.

In the last two columns, I study the impact of the factory collapse on the fraction of school-aged boys and girls who are enrolled in school. For both girls and boys, I again find a positive trend over time in the fraction enrolled, and do not observe any differences in the levels enrolled between sending and non-sending districts. While point estimates for fraction of girls and boys enrolled in school are positive (an increase of 0.015 and 0.022 percentage points, respectively), these changes are not precisely estimated. The wild cluster bootstrap test that the interaction term in each regression is equal to zero is large (0.28 for girls' outcomes, 0.19 for boys' outcomes). However, the effect sizes implied by the coefficients are much smaller than what I am comfortably powered to detect.

I then estimate a specification using survey year dummies and their interactions with indi-

cators for districts that have a history of sending female migrants to work in garment factories. I present these results in table 3.9. In column 1, I look at the number of 5 year old girls enrolled; I find that there is an increase in enrollment over time in both sending and non-sending districts, but again, there is no difference in the probability of enrollment between sending and non-sending districts. When I look at the coefficients on the interactions between survey years and the indicator for a sending district, I cannot reject either that all interaction terms are equal to one another, nor can I reject that all the interaction terms are equal to zero. The effect sizes implied by the coefficients are much smaller than the effect sizes I can comfortably detect. In the second column of table 3.9, I look at the number of 5 year old boys enrolled in school. I again find an increase in enrollment over time, and no difference in the overall level of enrollment between sending and non-sending districts. I find that there is a precisely estimated decrease in the number of boys enrolled in sending districts in 2007, but no other year and sending district interaction term is statistically significant. However, when I test whether the 2014 and 2018 sending district interaction terms are equal to the remainder of the interaction terms, I can reject that the number of boys enrolled in sending districts in 2014 was equal to the number enrolled in years prior to the shock. This effect—of an increase of 0.084 boys enrolled in sending districts in 2014—is close to a size that I am powered to detect. This result is consistent with results presented in table 3.8, and could indicate that the additional income earned by women who had prematurely returned from garment factories—or the money saved by not sending a migrant after the shock—could have facilitated school entry for boys. This effect appears to dissipate by 2018; the wild cluster bootstrap test of whether the interaction of a household living in a sending district and being surveyed in 2018 is equal to all the other year interaction terms is 0.12, showing that this coefficient is not statistically different from other years.

The last two columns of table 3.9 show that school enrollment rates for both school aged boys and girls were increasing over time, but that the share of children enrolled in school was lower in sending districts compared to non-sending districts. When I consider the survey year and sending district interaction terms, I see that the trend both before and after the factory collapse in enrollment rates for boys and girls was significantly different in sending districts: both boys' and girls' school enrollment rates were growing faster than the corresponding rates in non-sending districts. I also cannot reject that the effect in 2014 in sending districts is equal to the effect in other years, for both boys' and girls' enrollment. This pattern of coefficients could suggest that households were able to cope with the labor demand shock caused by the factory collapse, and were able to smooth consumption and continue investing in their children's education. It also indicates that parents' expectations regarding returns to education did not necessarily change in the aftermath of a large shock to labor demand; these households may have correctly perceived the event as causing a temporary, rather than a permanent, shock to women's employment. However, given the lack of parallel pre-trends in older children's education outcomes between sending and non sending districts over time, these coefficients should not be interpreted as causal.

3.5.5 Wages

Table 3.10 presents results on both the nominal daily agricultural wage for men and women, as well as results adjusted for yearly inflation using the rural CPI reported by the Bangladesh Bureau of Statistics. In columns 2 and 4, I find evidence of a small increase in the value of women's total compensation (monetary and non-monetary) in both sending and non-sending districts over time. I do not find that there is a difference in the wage paid to women in sending vs non-sending

districts, nor do I find that the factory collapse is associated with a change in women's agricultural wages in sending districts. Both conventional p-values and those estimated using a wild cluster bootstrap indicate that the interaction term in each column is statistically indistinguishable from zero.

In table 3.11, I estimate the impact of the factory shock on women's agricultural wages, this time using year dummies instead of an indicator for the wage observation coming from a year after the shock. For all measures of wages, I find a strong increase in wages over time in both sending and non-sending districts. I do not find that there is a level difference in wages between sending and non-sending districts, regardless of the measure of wages used. Interestingly, I find that none of the year-sending district interaction terms are statistically significant, nor can I reject that the interaction terms are all equal to each other. This is true, regardless of the measure of wages. These results imply that there was no differential wage changes in sending districts over time relative to wages in non-sending districts.

Barring incredibly high wage elasticities of labor supply, I am underpowered to detect reasonable changes in the wage rate. Even if I were powered to precisely detect small changes in the wage, not finding any clear impact is consistent with the fact that there are significant labor market frictions in Bangladesh. If the data I use represent minimum wages established for efficiency, to ensure minimum nutrition, fairness, or through local government policy, this could cause unemployment in agricultural wage markets. I am unable to observe the level of unemployment, or the level of labor used in household production. Both of these measures would have shed light on whether labor market frictions precluded women from finding jobs upon returning from the garment sector.

Among all the different specifications I estimate to determine the change in household

composition, I find approximately a 16% increase in the number of working age women within households in sending districts, though this is not necessarily precisely estimated or different than the increase in sending districts in other years. I use this result to calculate the approximate elasticity of labor demand in sending districts. First, I calculate the minimum change in wages I am powered to detect using monthly wage data at the greater district level. I estimate this in two ways: first assuming that the shock was temporary and would only impact wages in 2013, and then assuming the change persisted for all years following the collapse (until 2016, when the data ends). I account for clustering of wages within the region level, which is quite high. As shown in table 3.5, I am powered to detect changes bigger than 14.7 to 25.5% of the daily agricultural wage depending on the persistence of the shock. Combined with my results on household composition which approximates a change in labor supply assuming no supply side frictions—that all women returning from factories would be willing to work in unskilled agriculture labor—this would imply high wage elasticities of labor supply, between -0.9 and -1.57.

3.6 Conclusion

I find that the 2013 Rana Plaza factory disaster resulted in an increase of 0.032 more working age women returning to live in their natal households in the year 2014. This represents a 16.32% increase in the number of working age women in districts that have historically sent female migrants to work in garment factories. This shock appears temporary, as numbers return to their previous levels the next year, the last year for which I have data. These figures are underestimates if we believe that some proportion of returnees would either marry and form separate households, or would live in another extended family member's household outside the district. I

find evidence suggesting that some proportion of returnees married in response to their job loss; I find a 0.013 percentage point increase in the probability that a daughters-in-law lives in the household in sending districts in 2014. However, this result is attenuated by my findings for unmarried working age women in residence. I see no evidence that investment in girls' or boys' education changed in response to the shock, though results suggest that there was an increase of 0.08 percentage points in the enrollment rates of 5 year old boys. This could suggest that households were able to increase enrollment for boys, who have lower relative returns to education in sending districts compared to their sisters who can ultimately work in garment factories, using earnings from women who returned from Dhaka, or savings from not sending a female migrant. However, this finding could also be the result of other factors at play in sending districts after the shock. Ultimately, my results suggest that households were both able to smooth consumption by borrowing or reallocating income, and that they perceived the sector would rebound. Given that the number of working age women in sending districts appeared to return to pre-shock levels in 2018, the last year in which I have data, this perception appears to be validated.

I find no evidence that the collapse affected the daily agricultural wage in sending districts, though I am not powered to precisely estimate the small changes that would be proportional to the small change in female migration that I observe. This could be because skilled, former RMG workers are not substitutes for unskilled agricultural laborers, so excess supply of their labor in their natal districts does not affect the equilibrium wage, or because of wage rigidity for a variety of reasons (minimum wages for fairness or efficiency, local regulations). I am also unable to assess whether returnee migrants (over) supply labor to their family's farm or businesses in the presence of labor market frictions and associated separation failures. I calculate the smallest effect I was powered to detect—between 14.7 and 25.5%—which, given the 16.32% increase in

women in sending districts, implies an elasticity of labor demand of between -0.9 and -1.57. It is important to note several things: my results are sensitive to how sending districts are defined, and I do not have data on important margins of adjustment available to households. Finally, my results on changes in household composition are likely underestimates if endogenous household formation occurs.

The Rana Plaza disaster was an avoidable tragedy which claimed 1,131 lives, injured countless other individuals, and had significant economic consequences, of which we can only quantify a small fraction. However, my results suggest that the tragedy's larger effects were contained to the short run. Women were able to move relatively freely between Dhaka and their natal villages in response to new information regarding factory jobs, households were able to smooth their consumption, and households did not change their long run perceptions of the returns to women's education.

3.7 Tables & Figures

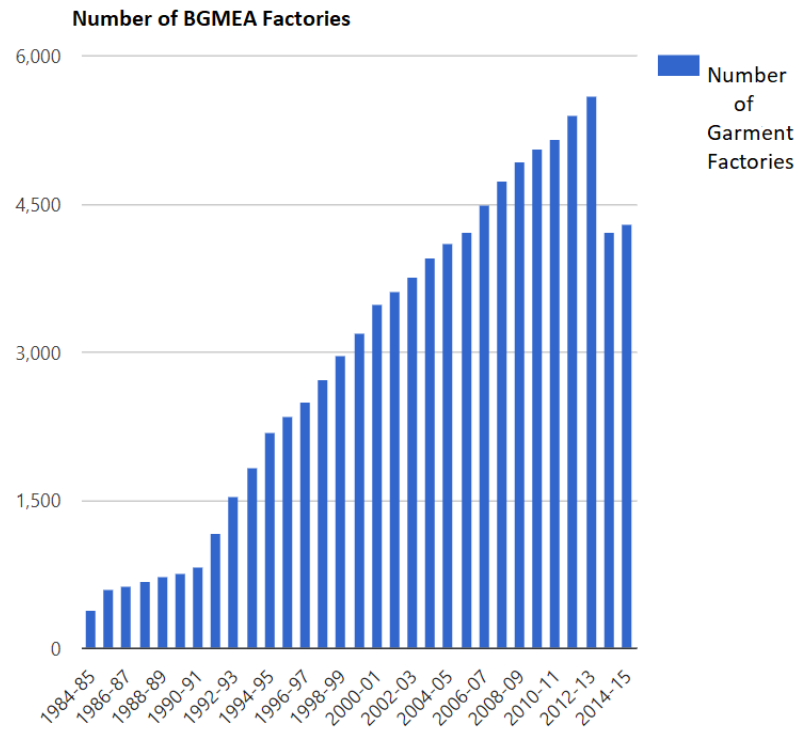


Figure 3.1: Number of RMG Factories in BGMEA per Year

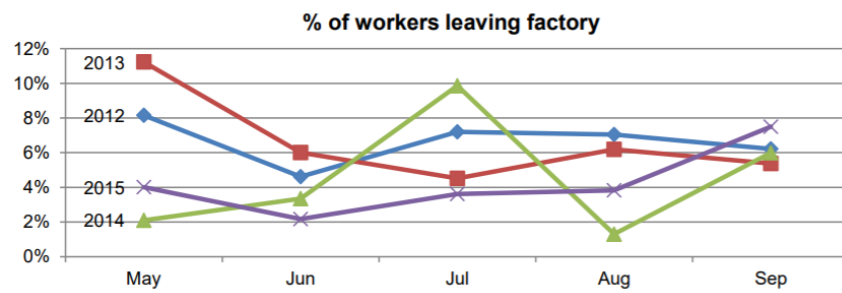


Figure 3.2: Monthly Migration Rate of RMGPP Operators

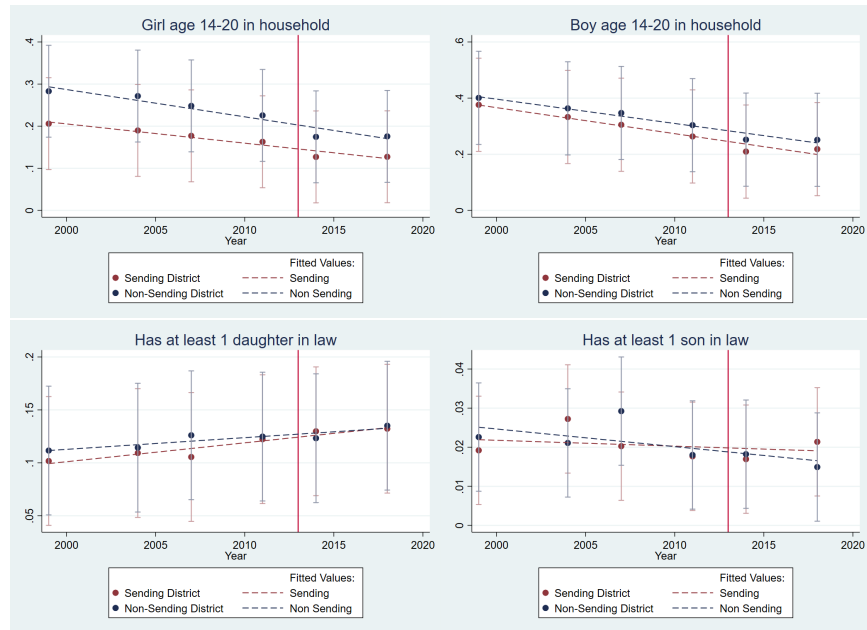


Figure 3.3: Parallel Trends, Household Composition

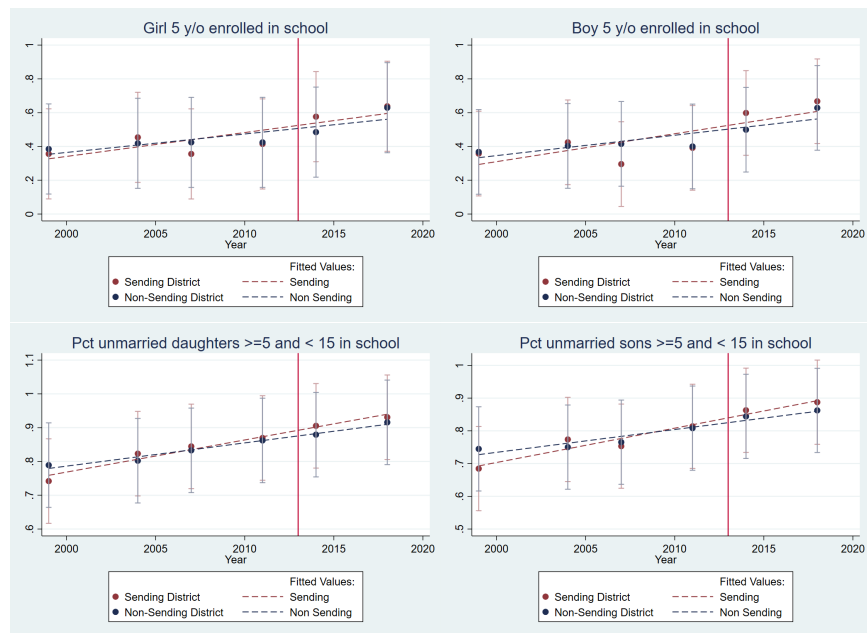


Figure 3.4: Parallel Trends, Education

Table 3.1: Summary Statistics

	Non Sending District	Sending District	Total
Head of household age	44.79 (14.01)	45.27 (14.31)	44.91 (14.09)
Spouse of head of household age	36.36 (11.96)	36.74 (12.31)	36.45 (12.05)
Age difference between household head and spouse	7.481 (6.234)	7.368 (5.956)	7.454 (6.168)
Head of household has any education	0.612 (0.487)	0.552 (0.497)	0.597 (0.490)
Spouse of head of household has any education	0.514 (0.500)	0.482 (0.500)	0.506 (0.500)
Household Size	4.740 (2.156)	4.419 (1.916)	4.662 (2.105)
Girl age 14-20 in household	0.219 (0.488)	0.163 (0.416)	0.205 (0.472)
Boy age 14-20 in household	0.315 (0.583)	0.274 (0.534)	0.305 (0.572)
Girl age 5-14 in household	0.268 (0.563)	0.236 (0.523)	0.260 (0.554)
Boy age 5-14 in household	0.280 (0.569)	0.257 (0.541)	0.275 (0.562)
Pct unmarried daughters ≥ 5 and < 15 in school	0.858 (0.326)	0.867 (0.320)	0.860 (0.324)
Pct unmarried sons ≥ 5 and < 15 in school	0.806 (0.371)	0.815 (0.365)	0.808 (0.369)
Girl 13 y/o enrolled in school	0.867 (0.339)	0.884 (0.321)	0.871 (0.335)
Boy 13 y/o enrolled in school	0.756 (0.429)	0.806 (0.396)	0.767 (0.422)
Average years education unmarried daughters ≥ 5 and < 15	3.067 (2.431)	2.942 (2.436)	3.038 (2.433)
Average years education unmarried sons ≥ 5 and < 15	2.748 (2.355)	2.696 (2.376)	2.735 (2.361)
Girl 5 y/o enrolled in school	0.459 (0.498)	0.468 (0.499)	0.461 (0.498)
Boy 5 y/o enrolled in school	0.433 (0.495)	0.476 (0.499)	0.443 (0.497)
Has at least 1 daughter in law	0.115 (0.319)	0.111 (0.314)	0.114 (0.318)
Has at least 1 son in law	0.0133 (0.114)	0.0137 (0.116)	0.0134 (0.115)
Religion: Islam	0.816 (0.388)	0.846 (0.361)	0.823 (0.381)
Religion: Hindu	0.100 (0.300)	0.0710 (0.257)	0.0932 (0.291)
Religion: Buddhist	0.0141 (0.118)	0.000103 (0.0101)	0.0107 (0.103)
Religion: Christian	0.00320 (0.0565)	0.00357 (0.0596)	0.00329 (0.0573)
Region: Barisal	0.103 (0.303)	0.105 (0.307)	0.103 (0.304)
Region: Chittagong	0.218 (0.413)	0 (0)	0.165 (0.371)
Region: Dhaka	0.185 (0.388)	0.160 (0.367)	0.179 (0.383)
Region: Khulna	0.196 (0.397)	0 (0)	0.148 (0.356)
Region: Mymensingh	0.0179 (0.133)	0.215 (0.411)	0.0658 (0.248)
Region: Rajshahi	0.0800 (0.271)	0.284 (0.451)	0.130 (0.336)
Region: Rangpur	0.0835 (0.277)	0.235 (0.424)	0.120 (0.325)
Region: Sylhet	0.117 (0.321)	0 (0)	0.0884 (0.284)

Table 3.2: Pre- Trends: Household Composition

	Woman 14 to 20	Any Daughter in Law	Any Son in Law
2004 Non Sending District	-0.011 (0.011)	0.006 (0.006)	-0.001 (0.003)
2007 Non Sending District	-0.035** (0.011)	0.023*** (0.006)	0.008* (0.003)
2011 Non Sending District	-0.057*** (0.010)	0.028*** (0.006)	-0.003 (0.002)
Sending District	-0.021 (0.016)	0.004 (0.009)	-0.004 (0.004)
2004 × Sending District	-0.002 (0.020)	0.006 (0.012)	0.009 (0.006)
2007 × Sending District	0.010 (0.020)	-0.005 (0.012)	-0.004 (0.006)
2011 × Sending District	0.016 (0.018)	0.009 (0.011)	0.004 (0.005)
Constant	0.217*** (0.014)	0.078*** (0.008)	0.016*** (0.004)
Region FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Wild Bootstrap P Value			
Year x Sending= 0	0.514	0.561	0.170
R^2	0.023	0.033	0.005
Observations	47611	47611	47611

Table 3.3: Pre- Trends: School Enrollment

	Pct Girls Enrolled 4 to 14	Pct Boys Enrolled 4 to 14	Pct Girls Enrolled Age 5	Pct Boys Enrolled Age 5
2004 Non Sending District	0.015 (0.013)	0.006 (0.012)	0.031 (0.037)	0.031 (0.035)
2007 Non Sending District	0.044*** (0.012)	0.015 (0.012)	0.032 (0.040)	0.018 (0.036)
2011 Non Sending District	0.062*** (0.012)	0.045*** (0.011)	0.013 (0.040)	-0.007 (0.033)
Sending District	-0.034 (0.022)	-0.035 (0.021)	-0.013 (0.064)	0.026 (0.050)
2004 × Sending District	0.068* (0.029)	0.084** (0.027)	0.060 (0.087)	0.009 (0.070)
2007 × Sending District	0.053* (0.026)	0.043 (0.027)	-0.033 (0.088)	-0.130* (0.064)
2011 × Sending District	0.058* (0.024)	0.065** (0.024)	0.022 (0.074)	-0.011 (0.067)
Constant	0.698*** (0.018)	0.626*** (0.020)	0.220** (0.073)	0.315*** (0.073)
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Wild Bootstrap				
P Value				
Year x Sending= 0	0.136	0.162	0.903	0.136
R^2	0.054	0.055	0.055	0.063
Observations	18508	19446	2226	2366

Table 3.4: Minimum Detectable Effects

	Sending N	Non-sending N	Treatment Mean	Control Mean	Std Dev	ICC	Effect Size % Mean
Woman 14 to 20 in Residence							
Persistent T v C	20089	58736	0.257	0.274	0.0765	0.707	0.0640
Persistent Pre v Post	20089	18860	0.236	0.290	0.0603	0.259	0.187
Temporary T v C	4321	58736	0.257	0.320	0.0765	0.707	0.200
Temporary Pre v Post	4321	18860	0.236	0.290	0.0603	0.259	0.188
Any daughter-in-law							
Persistent T v C	20089	58736	0.104	0.153	0.0594	0.0972	0.324
Persistent Pre v Post	20089	18860	0.107	0.132	0.0481	0.0807	0.184
Temporary T v C	4321	58736	0.104	0.131	0.0594	0.0972	0.211
Temporary Pre v Post	4321	18860	0.107	0.132	0.0481	0.0807	0.185
Any Son in Law							
Persistent T v C	20089	58736	0.0203	0.0251	0.0103	0.113	0.191
Persistent Pre v Post	20089	18860	0.0200	0.0275	0.0104	0.163	0.270
Temporary T v C	4321	58736	0.0203	0.0271	0.0103	0.113	0.251
Temporary Pre v Post	4321	18860	0.0200	0.0275	0.0104	0.163	0.271
Girl 5-14 in School							
Persistent T v C	20089	58736	0.808	0.867	0.0892	0.317	0.0682
Persistent Pre v Post	20089	18860	0.803	0.845	0.0819	0.0844	0.0500
Temporary T v C	4321	58736	0.808	0.851	0.0892	0.317	0.0502
Temporary Pre v Post	4321	18860	0.803	0.845	0.0819	0.0844	0.0501
Boy 5-14 in School							
Persistent T v C	20089	58736	0.761	0.808	0.0979	0.0979	0.0578
Persistent Pre v Post	20089	18860	0.750	0.788	0.0949	0.0512	0.0484
Temporary T v C	4321	58736	0.761	0.798	0.0979	0.0979	0.0458
Temporary Pre v Post	4321	18860	0.750	0.788	0.0949	0.0512	0.0486
Girl 5 years old in school							
Persistent T v C	20089	58736	0.390	0.436	0.123	0.371	0.105
Persistent Pre v Post	20089	18860	0.371	0.453	0.123	0.142	0.182
Temporary T v C	4321	58736	0.390	0.466	0.123	0.371	0.164
Temporary Pre v Post	4321	18860	0.371	0.453	0.123	0.142	0.182
Boy 5 years old in school							
Persistent T v C	20089	58736	0.365	0.439	0.120	0.218	0.169
Persistent Pre v Post	20089	18860	0.414	0.490	0.124	0.121	0.156
Temporary T v C	4321	58736	0.365	0.433	0.120	0.218	0.158
Temporary Pre v Post	4321	18860	0.414	0.490	0.124	0.121	0.156

Table 3.5: Minimum Detectable Effects on Wages & Implied Elasticity of Labor Demand

	Sending N	non-sending N	Treatment Mean	Control Mean	SD	ICC	Effect Size % Mean	Elasticity
Persistent T v C	384	924	83.79	109.9	43.78	0.123	0.238	-1.461
Persistent Pre v Post	384	1440	78.24	91.72	45.91	0.0193	0.147	-0.903
Temporary T v C	96	252	83.79	112.5	43.78	0.123	0.255	-1.569
Temporary Pre v Post	96	1440	78.24	95.80	45.91	0.0193	0.183	-1.127

Table 3.6: Individuals in HH: Post x Sending

	Unmarried Woman 14-20	Any daughter-in-law	Any Son in Law
Post	-0.081*** (0.006)	0.027*** (0.004)	-0.005** (0.001)
Sending District	-0.020* (0.008)	0.001 (0.005)	-0.002 (0.002)
Post × Sending District	0.024** (0.009)	0.013+ (0.007)	0.005+ (0.003)
Constant	0.197*** (0.009)	0.092*** (0.006)	0.019*** (0.003)
Region FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Control Mean	0.265	0.118	0.022
Wild Bootstrap			
P Value	0.039	0.009	0.065
R^2	0.025	0.035	0.005
Observations	84254	84254	84254

All columns include region fixed effects and controls for the age difference between the household head and their spouse, the education levels of the household head and their spouse, and dummy variables for the household's religion. Standard errors are presented in parentheses below point estimates. All specifications use cluster standard errors at the union level and use survey weights associated with each cross section. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.7: Individuals in HH: Year x Sending

	Women 14-20	Any Daughter in Law	Any Son in Law
2004	-0.012 (0.011)	0.006 (0.006)	-0.001 (0.003)
2007	-0.035** (0.011)	0.024*** (0.005)	0.008** (0.003)
2011	-0.055*** (0.010)	0.030*** (0.004)	-0.003 (0.002)
2014	-0.110*** (0.011)	0.033*** (0.005)	-0.003 (0.002)
2018	-0.106*** (0.010)	0.053*** (0.006)	-0.005* (0.002)
Sending District	-0.027+ (0.016)	-0.002 (0.011)	-0.004 (0.004)
2004 × Sending District	-0.002 (0.020)	0.006 (0.012)	0.009 (0.006)
2007 × Sending District	0.010 (0.021)	-0.005 (0.012)	-0.004 (0.006)
2011 × Sending District	0.016 (0.018)	0.009 (0.012)	0.004 (0.005)
2014 × Sending District	0.032+ (0.019)	0.020* (0.009)	0.004 (0.005)
2018 × Sending District	0.028 (0.017)	0.011 (0.012)	0.011* (0.005)
Constant	0.223*** (0.012)	0.076*** (0.007)	0.018*** (0.003)
Region FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Wild Bootstrap			
2014 x Sending	0.220	0.237	0.149
Wild Bootstrap			
2018 x Sending	0.241	0.510	0.018
Non Sending Mean Pre Shock	0.255	0.122	0.017
Sending Mean Pre Shock	0.193	0.114	0.015
R^2	0.026	0.036	0.005
Observations	84254	84254	84254

All columns include region fixed effects and controls for the age difference between the household head and their spouse, the education levels of the household head and their spouse, and dummy variables for the household's religion. Standard errors are presented in parentheses below point estimates. All specifications use cluster standard errors at the union level and use survey weights associated with each cross section. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.8: Children In School: Post x Sending

	Girls 5 y/o enrolled	Boys 5 y/o enrolled	% Girls ≥ 5 & < 15 in school	% Boys ≥ 5 & < 15 in school
Post	0.109*** (0.024)	0.121*** (0.024)	0.061*** (0.006)	0.063*** (0.008)
Sending District	-0.002 (0.036)	-0.005 (0.031)	0.009 (0.010)	0.010 (0.011)
Post × Sending District	0.039 (0.053)	0.091+ (0.047)	0.015 (0.013)	0.022 (0.014)
Constant	0.247*** (0.056)	0.355*** (0.057)	0.736*** (0.014)	0.656*** (0.015)
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Wild Bootstrap				
P Value	0.444	0.122	0.283	0.189
Control Mean	0.421	0.401	0.824	0.774
R^2	0.068	0.080	0.050	0.057
Observations	3522	3679	31073	32305

All columns include region fixed effects and controls for the age difference between the household head and their spouse, the education levels of the household head and their spouse, and dummy variables for the household's religion. Standard errors are presented in parentheses below point estimates. All specifications use cluster standard errors at the union level and use survey weights associated with each cross section. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.9: Children In School: Year x Sending

	Girls 5 y/o enrolled	Boys 5 y/o enrolled	% Girls ≥ 5 & < 15 in school	% Boys ≥ 5 & < 15 in school
2004	0.032 (0.037)	0.032 (0.035)	0.014 (0.013)	0.005 (0.012)
2007	0.029 (0.040)	0.020 (0.036)	0.045*** (0.013)	0.016 (0.012)
2011	0.011 (0.039)	-0.007 (0.033)	0.064*** (0.013)	0.046*** (0.011)
2014	0.070+ (0.038)	0.075* (0.038)	0.082*** (0.012)	0.080*** (0.014)
2018	0.191*** (0.037)	0.195*** (0.035)	0.105*** (0.011)	0.081*** (0.010)
Sending District	-0.021 (0.063)	0.029 (0.048)	-0.035+ (0.021)	-0.039+ (0.021)
2004 × Sending District	0.062 (0.088)	0.006 (0.070)	0.068* (0.029)	0.085** (0.027)
2007 × Sending District	-0.028 (0.087)	-0.132* (0.064)	0.053* (0.026)	0.042 (0.027)
2011 × Sending District	0.030 (0.074)	-0.011 (0.066)	0.057* (0.024)	0.065** (0.024)
2014 × Sending District	0.095 (0.088)	0.084 (0.077)	0.064* (0.025)	0.067* (0.027)
2018 × Sending District	0.025 (0.081)	0.018 (0.069)	0.056* (0.023)	0.076** (0.023)
Constant	0.227*** (0.062)	0.333*** (0.061)	0.708*** (0.016)	0.642*** (0.017)
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Wild Bootstrap				
2014 x Sending Wild Bootstrap	0.577	0.040	0.940	0.457
2018 x Sending	0.794	0.119	0.961	0.361
Non Sending Mean Pre Shock	0.421	0.401	0.824	0.774
Sending Mean Pre Shock	0.427	0.383	0.838	0.772
R^2	0.073	0.086	0.057	0.061
Observations	3522	3679	31073	32305

All columns include region fixed effects and controls for the age difference between the household head and their spouse, the education levels of the household head and their spouse, and dummy variables for the household's religion. Standard errors are presented in parentheses below point estimates. All specifications use cluster standard errors at the union level and use survey weights associated with each cross section. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.10: Daily Agricultural Wage Rate for Unskilled Workers,
Post x Sending

	Women Nominal Wage	Women Nominal Wage With Food	Women Real Wage	Women Real Wage With Food
After Collapse	-26.39 (27.14)	16.65+ (7.395)	-27.15 (26.78)	15.79+ (7.307)
Sending District	0.343 (5.062)	9.000 (5.708)	0.205 (5.075)	9.076 (6.013)
After Collapse $\times$ Sending District	-1.833 (10.38)	-8.279 (11.20)	-1.614 (10.17)	-8.424 (10.91)
Constant	49.99*** (4.570)	31.67*** (5.113)	53.40*** (4.141)	33.97*** (4.867)
Region FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Wild Bootstrap P Value	0.883	0.563	0.914	0.555
R^2	0.731	0.903	0.721	0.896
Observations	4246	3700	4053	3519

All columns include region fixed effects and standard errors clustered at the region level are presented in parentheses below point estimates. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.11: Daily Agricultural Wage Rate for Unskilled Workers,
Year x Sending

	Women Nominal Wage	Women Nominal Wage With Food	Women Real Wage	Women Real Wage With Food
1999	3.373** (0.767)	2.007 (1.469)		
2000	5.717** (1.113)	4.237* (1.772)	2.735+ (1.267)	3.053*** (0.526)
2001	7.101** (1.540)	5.840* (2.367)	4.008** (1.134)	4.659* (1.424)
2002	10.46*** (1.438)	8.478+ (4.451)	7.254*** (1.310)	6.949+ (3.580)
2003	13.95*** (1.215)	9.601+ (4.973)	10.68*** (1.338)	8.012 (4.610)
2004	14.20*** (1.398)	8.850* (2.837)	10.95*** (1.675)	7.339* (2.314)
2005	19.30*** (1.353)	15.27*** (2.340)	15.96*** (2.044)	13.77*** (1.432)
2006	24.39*** (2.684)	20.07*** (3.277)	21.13*** (2.898)	18.53*** (2.465)
2007	35.38*** (0.970)	30.05*** (1.600)	31.75*** (1.597)	28.28*** (0.708)
2008	56.32*** (1.980)	44.53*** (2.604)	52.89*** (2.362)	43.04*** (2.153)
2009	71.63*** (8.067)	57.97*** (2.165)	67.97*** (7.731)	56.28*** (1.272)
2010	82.67*** (3.993)	72.95*** (3.126)	78.87*** (4.050)	71.15*** (2.894)
2011	122.9***	98.61***	118.6***	96.49***

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	Women Nominal Wage	Women Nominal Wage With Food	Women Real Wage	Women Real Wage With Food
2012	(5.581) 143.4***	(6.176) 116.6***	(5.195) 139.3***	(5.347) 114.6***
2013	(5.155) 177.2***	(6.320) 142.2***	(4.704) 172.2***	(5.431) 139.6***
2014	(12.61) 183.4***	(13.55) 170.6***	(11.88) 179.5***	(12.91) 168.6***
2015	(9.910) 189.1***	(11.32) 175.4***	(9.411) 185.1***	(10.42) 173.5***
2016	(10.65) 218.0***	(12.00) 200.6***	(10.06) 214.2***	(11.05) 198.7***
Sending District	(11.17) 3.072	(12.47) 7.822	(10.34) 2.764	(11.81) 11.17
1999 × Sending District	(10.12) -0.353	(8.311) 3.106+	(10.27) (10.27)	(8.390)
2000 × Sending District	(0.795) -2.223+	(1.373) -0.981	-1.904 (1.417)	-4.309 (3.426)
2001 × Sending District	(1.096) -1.800	(4.032) 3.286	-1.374+ (0.671)	0.204 (1.446)
2002 × Sending District	(1.324) -4.913	(2.326) 5.179	(0.671) -4.533	(1.446) 2.487
2003 × Sending District	(2.895) -7.423*	(5.140) 1.472	(2.774) -7.073+	(4.485) -1.486
2004 × Sending District	(3.105) -5.335	(5.167) 2.025	(3.081) -4.974	(5.034) -1.134
2005 × Sending District	(3.472) -4.751	(2.829) -3.607	(3.729) -4.422	(2.312) -6.882+
2006 × Sending District	(3.687) -4.570	(3.533) -3.259	(4.036) -4.438	(2.926) -6.624
	(4.364)	(4.619)	(4.519)	(4.231)

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	Women Nominal Wage	Women Nominal Wage With Food	Women Real Wage	Women Real Wage With Food
2007 × Sending District	-0.816 (5.630)	-1.553 (4.716)	-0.564 (5.889)	-4.932 (4.334)
2008 × Sending District	-6.242 (4.894)	-1.202 (5.192)	-5.943 (5.112)	-4.547 (5.157)
2009 × Sending District	-8.985 (10.72)	3.026 (4.627)	-8.686 (10.59)	-0.361 (4.504)
2010 × Sending District	-3.021 (11.61)	5.392 (9.094)	-2.793 (11.79)	1.925 (8.953)
2011 × Sending District	6.627 (14.44)	4.235 (11.60)	6.781 (14.64)	0.626 (11.15)
2012 × Sending District	11.47 (16.66)	4.664 (12.59)	11.66 (16.66)	1.171 (12.06)
2013 × Sending District	-1.017 (18.99)	-0.305 (18.10)	-0.481 (18.66)	-3.695 (17.60)
2014 × Sending District	-7.616 (15.63)	-7.121 (16.75)	-7.214 (15.52)	-10.53 (16.17)
2015 × Sending District	-2.423 (15.61)	-4.331 (13.87)	-2.084 (15.52)	-7.755 (13.39)
2016 × Sending District	-13.43 (15.15)	-14.45 (15.21)	-13.06 (14.65)	-17.84 (14.81)
Constant	50.72*** (6.666)	31.49*** (6.050)	54.43*** (6.400)	32.62*** (5.489)
Region FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Wild Bootstrap P Value				
2014 = All Pre Shock	0.242	0.852	0.656	0.883

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	Women Nominal Wage	Women Nominal Wage With Food	Women Real Wage	Women Real Wage With Food
Wild Bootstrap P Value				
2015 = All Pre Shock	0.984	0.281	0.984	0.266
Wild Bootstrap P Value				
2016 = All Pre Shock	0.258	0.555	0.406	0.523
Non Sending Mean Pre Shock	88.916	88.916	91.612	91.612
Sending Mean Pre Shock	87.741	87.741	89.670	89.670
R^2	0.731	0.903	0.721	0.896
Observations	4246	3700	4053	3519

All columns include region fixed effects and standard errors clustered at the region level are presented in parentheses below point estimates. $+p < 0.10$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$

Appendix A: Consanguinity and Marital Transfers in Egypt

A.1 Supplementary Tables

Table A1.1: Sample Composition

		Year Last Lived With Parents			
Individual Last Observed		1998	2006	2012	2018
	2006	215	76	0	0
	2012	0	494	115	0
	2018	544	1,553	2,786	434

Each cell shows how many of the individuals who were last surveyed in the round indicated by the row last lived with their parents in the survey round indicated by the column

Table A1.2: Parent Sample Composition

		Parent Info Source	
Individual Last Lived With Family		2012	2018
	1998	759	0
	2006	2,123	0
	2012	2,901	0
	2018	0	434

Each cell shows which survey round was the source of parents' information given that the last time the individual was observed living with their parents was the round indicated by a particular row.

Table A1.3: Father's Birth Order & Siblings: Education & Wealth

	Wealth			Any Education		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of relevant father siblings	0.0108 (0.475)	-0.00895 (0.489)	-0.0121 (0.316)	0.001000 (0.767)	-0.000738 (0.818)	-0.000616 (0.843)
Constant	-0.914 (0.000)***	73.59 (0.000)***	36.88 (0.000)***	0.258 (0.000)***	-15.41 (0.000)***	-17.27 (0.000)***
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Father Controls	Yes	Yes	Yes	Yes	Yes	Yes
Geography		Yes	Yes		Yes	Yes
Year of Birth			Yes			Yes
Education			Yes			Yes
Obs	6,205	6,061	5,960	6,061	6,061	5,960

Results estimated by OLS, clustering standard errors at the father's household level. +p<.1, *p<.05, ** p<.01, *** p<.001

Table A1.4: Father's Birth Order, Wealth: First Stage Test

	(1)	(2)
Female=0 $\times$ Asset index	-0.0136 (0.000)***	-0.0134 (0.000)***
Female=1 $\times$ Asset index	-0.00372 (0.282)	-0.00376 (0.277)
Constant	-2.783 (0.054)*	-2.857 (0.048)**
Gender	Yes	Yes
Father Controls	Yes	Yes
Siblings	Yes	Yes
Geography	Yes	Yes
Year of Birth	Yes	Yes
Education	Yes	Yes
F Statistic	6.005	5.960
Obs	5,964	5,955

Standard errors clustered at the father's level. +p<.1,

*p<.05, ** p<.01, *** p<.001

Table A1.5: Father's Birth Order, Siblings: Number of Children

	# Sons (1)	# Daughters (2)	# Children (3)
Father's birth order	0.0109 (0.402)	0.0165 (0.193)	0.0274 (0.100)*
Constant	4.350 (0.000)***	1.226 (0.000)***	5.576 (0.000)***
Geography	Yes	Yes	Yes
Year of Birth	Yes	Yes	Yes
Father Characteristics	Yes	Yes	Yes
Obs	4,558	4,558	4,558

Robust standard errors. +p<.1, *p<.05, ** p<.01, *** p<.001

A.2 Women's Empowerment Robustness Checks

Table A2.6: Woman Has Say in Decisions About, I

	Household Food	Own Medical Care	Own Clothes	Child Medical Care	Child School
Andrews & Armstrong Coefficient	1.254 (0.229) (0.035)	1.191 (0.240) (0.030)	0.686 (0.424) (0.256)	-0.004 (0.995) (0.990)	0.444 (0.515) (0.412)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	0.47	0.44	0.26	0.00	0.17
Fuller Coefficient	1.175 (0.107)	1.117 (0.107)	0.644 (0.307)	-0.004 (0.993)	0.412 (0.427)
2SLS Coefficient	1.498 (0.151)	1.438 (0.156)	0.830 (0.333)	-0.008 (0.990)	0.523 (0.443)
OLS Coefficient	-0.014 (0.440)	-0.008 (0.632)	-0.009 (0.600)	0.007 (0.630)	0.015 (0.322)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.010	0.000	0.000	0.000	0.010
N	5,949	5,936	5,937	5,934	5,928
Kleibergen-Paap F	3.787	3.592	3.604	3.627	3.672
Exsanguineous Mean	0.610	0.310	0.520	0.270	0.270

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence.

Table A2.7: Woman Has Say in Decisions About, II

	Child Clothes	Large HH Purchases	Daily HH Purchases	Visit Family & Friends	Using HH Money
Andrews & Armstrong Coefficient	0.300 (0.673) (0.600)	0.185 (0.738) (0.681)	0.051 (0.943) (0.921)	0.895 (0.283) (0.079)	-0.312 (0.655) (0.579)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	0.11	0.07	0.02	0.33	-0.12
Fuller Coefficient	0.281 (0.607)	0.169 (0.692)	0.050 (0.928)	0.833 (0.155)	-0.297 (0.584)
2SLS Coefficient	0.360 (0.613)	0.222 (0.688)	0.070 (0.921)	1.077 (0.196)	-0.373 (0.594)
OLS Coefficient	0.001 (0.929)	-0.018 (0.145)	-0.022 (0.185)	-0.025 (0.088)	-0.019 (0.251)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	0.000	-0.010	-0.010	-0.010	-0.010
N	5,935	5,932	5,935	5,935	5,949
Kleibergen-Paap F	3.625	3.619	3.637	3.607	3.787
Exsanguineous Mean	0.340	0.180	0.430	0.250	0.670

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence.

Table A2.8: Locations Women Can Physically Travel Alone

	Market	Own Medical Care	Child Medical Care	Visit Friends
Andrews & Armstrong Coefficient	-0.894 (0.261) (0.073)	-0.157 (0.825) (0.798)	-0.590 (0.438) (0.315)	0.141 (0.795) (0.754)
Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.33	-0.06	-0.22	0.05
Fuller Coefficient	-0.845 (0.139)	-0.145 (0.795)	-0.549 (0.340)	0.135 (0.751)
2SLS Coefficient	-1.064 (0.181)	-0.181 (0.799)	-0.693 (0.362)	0.168 (0.757)
OLS Coefficient	-0.039 (0.009)	-0.013 (0.443)	-0.022 (0.202)	0.012 (0.347)
OLS Effect of 1 Std Dev Increase of Prob Cousin Marriage	-0.010	0.000	-0.010	0.000
N	5,949	5,949	5,949	5,949
Kleibergen-Paap F	3.787	3.787	3.787	3.787
Exsanguineous Mean	0.800	0.610	0.610	0.840

The first panel presents point estimates and p-values using Andrews and Armstrong [11] unbiased estimator, with standard p-values in parentheses below. P-values from the Anderson-Rubin test are bolded in parentheses underneath, and the last row of the first panel shows the effect of a one standard deviation increase in the probability of cousin marriage using the point estimate from the Andrews & Armstrong unbiased estimator. The second panel presents point estimates and p-values using the Fuller estimator ($\alpha = 1$), and the third panel presents point estimates and p-values using 2SLS. Panel four presents point estimates and p-values using OLS, as well as the effect of a one standard deviation increase in the probability of cousin marriage using the OLS point estimate. Divide point estimates by 100 to obtain the effect of a one percentage point increase in the probability of cousin marriage. Rate of consanguineous marriage in sample is 16.7%. All columns include controls for fathers' siblings, education, year of birth, and wealth; the individuals' siblings, year of birth, and education; and indicators for geographic residence.

Appendix B: Jobs and Intimate Partner Violence: Evidence from Bangladesh

B.1 Incentivized Decision Making Activity 1 Script

Enumerator Read: *I am going to ask you a few questions. Each question will ask you to choose between two options. Consider each question separately. Think carefully about your decision. At the end of the survey, we may randomly choose one decision you make and pay your household accordingly. Your spouse will NOT know how you allocated this money*

Question 1 Enumerator Read: *Option A: I give you 110 taka directly to you. Option B: I give 110 taka directly to [SPOUSE NAME]. Please note that this money cannot be divided. Either I will give all the money to you or give all the money to [SPOUSE NAME].*

If the respondent chooses option A, [VALUE SELF] = 100 taka, and [VALUE SPOUSE]=110 taka. If the respondent chooses option B, [VALUE SELF] = 110 taka, and [VALUE SPOUSE]=100 taka.

Question 2: *Option A: I give [VALUE SELF] taka directly to you. Option B: I give [VALUE SPOUSE] directly to [SPOUSE]. Please note that this money cannot be divided. Either you will keep all the money to yourself or give all the money to [SPOUSE NAME]*

If the respondent switches the transfer recipient, the activity ends. If the respondent does not switch, question 2 repeats up to 9 more times, each time removing 10 taka from the pot given to the person the respondent chose to give the transfer. The activity stops when the respondent switches the transfer recipient or when the pot given to the respondent's preferred recipient goes to zero.

B.2 Incentivized Decision Making Activity 2 Script

Enumerator Read: *Thank you. Now, we are going to do another activity. This activity is NOT related to the activity we just completed. Suppose I would like to give your household 110 taka. Of this 110 taka, how much would you like me to give you directly, and how much would you like me to give to your spouse? Please ensure your answer is in increments of 10 taka. For example, you should choose to have me give 10 taka directly to you, and 100 taka directly to your spouse. You could not choose for me to give you 5 taka and give 105 taka to your spouse. Again, think carefully about your decision, as this may be the choice we select for payment! Your spouse will NOT know how you allocated this money.*

Enumerator Instructions: If the respondent has not indicated she is ready after 2 minutes,

gently ask the respondent if they have made a decision. If so, continue with the script. If not, give the respondent more time and repeat after 2 minutes.

Enumerator Read: *How much money would you like to receive directly? How much money would you like your spouse to receive directly?*

B.3 List Randomization Protocol

Enumerator Read Aloud: *“I am going to read out a list of things that people experience in their lives. I would like you to take your right hand and make a fist behind your back. For each item that I read out that you agree with, please put out one finger. Do not put out a finger if you do not agree with the sentence I read out. After I am done reading all the statements, please show me the number of fingers you have out on your right hand. I will not know which items apply to you. I will only know the total number of items that apply to you.”*

Enumerator Read Aloud: *“I would like to practice this way of answering questions once with you. Please make a fist with your right hand, but PLACE IT IN FRONT OF YOU. Now I will read out a list of things that are true for some people. Please put out a finger for each item that is true for you.”*

Practice List

- I ate rice yesterday
- My husband works outside the house
- I watch television
- I rode the bus yesterday
- I have more than 2 children

Enumerator Read Aloud: *“Now let us do the same thing, but this time put your hand BEHIND your back.”*

Enumerator Instructions: Once the respondent has her hand behind her back, continue.

Enumerator Read Aloud: *“Good. Now for each item that I read out that you agree with, please put out one finger. Do not put out a finger if you do not agree with the sentence I read out. After I am done reading all the statements, please show me the number of fingers you have out on your right hand. I will not know which items apply to you. I will only know the total number of items that apply to you.”*

List

- I am happy with my life
- I live with my in-laws family
- I help manage my family’s income
- My children go to school
- **My husband has ever kicked, slapped, or punched me**

Only 50% of the sample is read the last item on the list. Any difference in the number of items reported between the group that is read 4 items and the group that is read 5 items reflects the prevalence of IPV.

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